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NEW EXPERIMENTS Physico-Mechanicall Touching the Air.

The Elasticity of y^e Air. R: Boyle

NEW EXPERIMENTS

Physico-Mechanicall,

Touching

The Spring of the Air,

and its Effects,

(Made, for the most part, in a New *PNEUMATICAL ENGINE*)

Written by way of Letter

To the Right Honorable *Charles* Lord Vicount of *Dungarvan*, Eldest Son to the Earl of *CORKE*.

By the Honorable *Robert Boyle* Esq;

OXFORD:

Printed by *H: Hall*, Printer to the University, for *Tho: Robinson*. 1660.

To the Reader.

ALthough the following Treatise being far more prolix then becomes a Letter, and then I at first intended it; I am very unwilling to encrease the already excessive bulk of the Book by a Preface, yet there are some particulars that I think my self oblig'd to take notice of to the Reader, as things, that will either concern him to know, or me to have known.

In the first place then: If it be demanded why I publish to the World a Letter, which by its Stile and diverse Passages, appears to have been written as well For, as To a particular Person; I have chiefly these two things to answer: The one, That the Experiments therein related, having been many of them try'd in the presence of Ingenious Men; and by that means having made

some noise among the Virtuosi (*insomuch that some of them have been sent into Foreign Countries, where they have had the luck not to be despis'd) I could not without quite tyring more then one* Amanuensis, *give out half as many Copies of them as were so earnestly desired, that I could not civilly refuse them. The other, That intelligent Persons in matters of this kinde perswaded me, that the publication of what I had observ'd touching the Nature of the Air, would not be useless to the World; and that in an Age so taken with Novelties as is ours, these new Experiments would be grateful to the Lovers of free and real Learning: So that I might at once comply with my grand Design of promoting Experimental and Useful Philosophy, and obtain the great satisfaction of giving some to ingenious Men; the hope of which, is, I confess, a temptation that I cannot easily resist.*

Of my being somewhat prolix in many of my Experiments, I have these Reasons to render, That some of them being altogether new, seem'd to need the being circumstantially related, to keep the Reader from distrusting them: That divers Circumstances I did here and there set down for fear of forgetting them, when I may hereafter

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have occasion to make use of them in my other Writings: That in divers cases I thought it necessary to deliver things circumstantially, that the Person I addressed them to, might without mistake, and with as little trouble as is possible, be able to repeat such unusual Experiments: and that after I consented to let my Observations be made publick, the most ordinary Reason of my prolixity was, That foreseeing that such a trouble as I met with in making those tryals carefully, and the great expence of time that they necessarily require, (not to mention the charges of making the Engine, and imploying a man to manage it) will probably keep most men from trying again these Experiments; I thought I might doe the generality of my Readers no unacceptable peice of service, by so punctually relating what I carefully observ'd, that they may look upon these Narratives as standing Records in our new Pneumaticks, and need not reiterate themselves an Experiment to have as distinct an Idea of it, as may suffice them to ground their Reflections and Speculations upon.

And because sometimes 'tis the Discourse made upon the Experiment that makes it appear prolix, I have commonly left a conspicuous

spicuous interval betwixt such Discourses, and the Experiments whereunto they belong, or are annexed; that they who desire onely the Historical part of the account we give of our Engine, may read the Narratives, without being put to the trouble of reading the Reflections too: Which I here take notice of, for the sake of those that are well vers'd in the New Philosophy, and in the Mathematicks; that such may skip what was design'd, but for such Persons as may be less acquainted even then I, with matters of this nature (scarce so much as mention'd by any Writer in our Language) and not for them from whom I shall be much more forward to learn, then to pretend to teach them. Of my being wont to speak rather doubtfully, or hesitantly, then resolvedly, concerning matters wherein I apprehend some difficulty, I have in another Treatise (which may, through Gods Assistance, come abroad ere long) given a particular, and I hope a satisfactory account: Wherefore I shall now defend my Practice but by the Observation of Aristotle, *who somewhere notes, That to seem to know all things certainly, and to speak positively of them, is a trick of bold and yong Fellows: Whereas those that are in-*

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deed intelligent and considerate, are wont to imploy more wary and diffident Expressions, or (as he speaks) προστιθέασιν ἀεὶ τὸ ἴσως, καὶ τὸ τάχα.

There are divers Reflections, and other Passages in the following Epistle, and even some Experiments (occasionally mention'd) which may seem either impertinent or superfluous, but are not so: Being purposely written, either to evince some truth oppos'd, or disprove some erroneous conceit maintain'd, by some eminent New Philosopher, or by some other Ingenious Men, who, I presum'd, would easily forgive me the having on such occasions purposely omitted their Names; though an inquisitive Person will probably discover divers of them, by the mention of the Opinions disprov'd in the Experiments I am excusing.

Ever since I discern'd the usefulness of speculative Geometry to Natural Philosophy, the unhappy Distempers of my Eyes, have so far kept me from being much conversant in it, that I fear I shall need the pardon of my Mathematical Readers, for some Passages, which if I had been deeply skill'd in Geometry, I should have treated more accurately.

And

And indeed, having, for Reasons elsewhere deduc'd, purposely kept my self a stranger to most of the new Hypotheses *in Philosophy, I am sensible enough that the Engine I treat of has prevail'd with me to write of some subjects which are sufficiently remote from those I have been most conversant in. And having been reduc'd to write the greatest part of the ensuing Letter at a distance, not onely from my Library, but from my own Manuscripts, I cannot but fear that my Discourses do not onely want many choice things wherewith the Learned Writings of others might have enriched or imbellished them: But that partly for this Reason, and partly for that touch'd upon a little before, It is possible I may have mention'd some Notions already publish'd by others, without taking notice of the Authors, not out of any design to defraud deserving Men, but for want of knowing such particulars to have been already publish'd by them: Especially the Experiments of our Engine being themselves sufficient to hint such Notions as we build upon them.*

The order of the Experiments every Reader may alter, as suits best with his own Design in perusing them: For not onely all

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those betwixt whom there is an Affinity in Nature (by belonging to one ſubject) are not always plac'd one by another, but they are not ſtill ſet down ſo much as in the order wherein they were made; but moſt commonly in that caſual one wherein my occaſions induc'd me to diſpatch them to the Preſs. And, which is worſe, I did uſually ſend quite away the former Experiments, before the later were written, or perhaps ſo much as made: Whereby I loſt the advantage of correcting and ſupplying the Imperfections of what I had formerly written, by the light of my ſubſequent Tryals and Diſcoveries.

Beſides all this, the diſtemper in my eyes forbidding me not onely to write my ſelf ſo much as one Experiment, but even to read over my ſelf what I dictated to others. I cannot but fear, that beſides the Authors miſtakes, this Edition may be blemiſh'd by many, that may be properly imputed to a very unskilful Writer (whom I was often times by haſte reduc'd againſt my cuſtom to imploy) and may have eſcaped the Diligence of that Learned Friend, that does me the favor to over-ſee the Preſs; eſpecially there being the diſtance of two days Journey betwixt it and me.

I need not perhaps repreſent to the equi-

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table Reader, how much the strange Confusions of this unhappy Nation, in the midst of which I have made and written these Experiments, are apt to disturb that calmness of Minde, and undistractedness of Thoughts, that are wont to be requisite to Happy Speculations. But I presume, that by all these things put together, he will readily perceive, That I have been so far from following the Poets prudent Counsel touching the slow Publication of Books design'd to purchase credit by,

—— Nonumque prematur in Annum

that I suffer this Treatise to come abroad into the World with a multitude of Disadvantages.

But if it be demanded, why then I did not make it fitter for the Press before I sent it thither? my Answer must be, That not at first imagining that this sort of Experiments would prove any thing near so troublesome, either to make, or to Record, as I afterwards found them, I did, to engage the Printer to dispatch, promise him to send him the whole Epistle in a very short time: So that although now and then the occasional vacations of the Press, by reason of Festivals,

vals, or the absence of the Corrector, gave me the leisure to exspaciate upon some subject; yet being oftentimes call'd upon to dispatch the Papers to the Press, my promise, and many unexpected Avocations, obliged me to a haste, which, though it have detracted nothing from the Faithfulness of the Historical part of our Book, has (I fear) been disadvantageous enough to all the rest. And I made the less scruple to let the following Papers pass out of my hands, with all their Imperfections; because, as the publick Affairs, and my own, were then circumstanc'd, I knew not when (if at all) I should be again in a condition to prosecute Experiments of this kinde; especially, since (to omit my being almost weary of being, as it were, confin'd to one sort of Experiments) I am pre-ingag'd (if it please God to vouchsafe me Life and Health) to imploy my first leisure in the publication of some other Physiological Papers, which I thought 'twould make me much the fitter to take in hand, if I first dispatch'd all that I had at this time to write touching our Engine.

I have this further to adde, by way of Excuse, That as it has been my design in publishing these Experiments to gratifie

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Ingenious men; so, if I have not been muchflattered, I may hope that the various hints to be met with in the following Letter, will (at least) somewhat awaken mens thoughts, & excite them to new speculations (such as perhaps even inquisitive men would scarce else light upon) and I need not despair, that even the examination of such new Suspicions and Enquiries will hence also, at least Occasionally, be facilitated: I said Occasionally, because it being, as 'tis proverbially said, Facile Inventis addere. *It seems not irrational to expect, that our Engine it self, and divers of our Experiments, will be much promoted by the Industry of Inventive and Mathematical Wits, whose contrivances may easily either correct or supply, and consequently surpass many of those we have made use of. And, particularly, if Men by skill and patience can arrive both to evacuate such Receivers as ours, till there be no more Air left in them, then there seems to have remain'd in the Glasses made use of about the Magdeburgick Experiment (hereafter to be mentioned) and to keep out the Air for a competent while, the Usefulness and Discoveries of our Engine, will not be a little advanc'd. And perhaps that may belong to it, which I remem-*

member Seneca *ſpeaks of Nature*, Initiatos (*ſays he*) nos credimus, in Veſtibulo ejus hæremus: *For being now in a place where we are not quite deſtitute of moderately skilful Artificers, we have, ſince the Concluſion of the following Letter, made ſome Additions to our Engine, by whoſe help we finde (upon ſome new tryals) that we may be able, without much of new trouble, to keep the ambient Air out of the exhauſted Receiver for a whole day; and perhaps we ſhould be able to keep it out much longer, if before we ſhall have diſpatch'd ſome urgent Affairs, and publiſh'd ſome Papers for which a kinde of Promiſe is thought to make us Debtors to the Preſs, we could be at leiſure to proſecute ſuch Experiments, as may poſsibly afford a Supplement to the following Treatiſe, from which I ſhall now no longer detain the Reader.*

Friendly Reader,

I Know all Persons that have a publick Spirit for the Advancement of Learning, will think much that this piece came not out in a Language of more general Use, then this you see it now attir'd in; especially since the Excellent Noble Person, who is the Author, is known to be well able himself (being almost universally a Linguist) to have given it either the Old Latin, or

a *the*

the newer French Dress.

But if it be an Honor to a Language to be preferr'd, and this Honor breeds sometimes an Emulation, as anciently it did between the Greeks *and* Romans, *it cannot be thought unhandsome for an English Nobleman to have preferr'd his own: And it may be a sufficient Reason for the Gentry of Forein Parts to learn our Speech, or keep Interpreters, that they are sure to have for their requital, from many of our English Writers (as here from this piece) much curiously ingenious, and profitable Learning.*

But

But as to this particular (give me leave to use Words from a Story) Since the Mountain cannot come to *Mahomet*, *Mahomet* will go to the Mountain: *I mean thus; Because many witty Men, Persons of Honor and Estate especially, may be suppos'd to be able to make a better account, by employing their Studies and Time on Matter then Words, and so are justly impeded from learning Languages; And because (as I may judge) the noble Author is willing to oblige all Men, He has already provided, that this piece shall shortly be done into Latine, that so*

it may come home to divers worthy Persons in its Stream, who cannot travel to finde it out in its first Origine.

Having therefore leave so to do, I cannot forbear to give the World the Advertisement of this Latine Edition, lest some skilful Artist should take needless pains about a Work, which will, ere long (by Gods furtherance) be done to his Hands; For such unprofitable expences of Study have too frequently happened, and too much to the disadvantage of Learning, for want of a sufficient Correspondence and Intercourse between such as are exercised

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in the Mines of Wisdome.

This is all the trouble I shall at present give you: Nor shall I need minde thee, if you have a true gust for the Book you read, to have an honor and thankful regard to the Person that has favor'd us with the Communication of these his Tryals, & is manifestly so great a Patron and Friend to Experimental Learning, and all true Wisdom; for should you fail in this, you might deservedly be depriv'd of some other Observations on the same subject, which the Author, I heare, has made since the finishing of this Treatise.

I desire to be excused that I

doe

not make Excuses for the slowness of the Publication, hoping that the long expectation you have had of it, will enhance, and not diminish your delight in the enjoyment of a piece like to be, amongst the students in accurate Philosophy, of so generall acceptance. Farewel.

R: Sh.

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The

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TO THE LORD OF DUNGARVAN, My Honoured and Dear NEPHEW.

My Dear Lord,

RECeiving in your laſt from *Paris*, a deſire that I would adde ſome more Experiments to thoſe I formerly ſent You over: I could not be ſo much your Servant as I am, without looking upon that Deſire as a Command; and conſequently, without thinking my ſelf obliged to conſider by what ſort of Experiments it might the moſt acceptably be obey'd. And at the ſame

time, perceiving by Letters from ſome other Ingenious Perſons at *Paris*, that ſeveral of the *Virtuoſi* there, were very intent upon the examination of the Intereſt of the Ayr, in hindring the deſcent of the Quick-ſilver, in the famous Experiment touching a *Vacuum*: I thought I could not comply with your Deſires in a more fit and ſeaſonable manner, then by proſecuting and endeavoring to promote that noble Experiment of *Torricellius*: and by preſenting your Lordſhip an account of my attempts to illuſtrate a ſubject, about which, it's being ſo much diſcourſ'd of where you are, together with your inbred Curioſity, and love of Experimental Learning, made me ſuppoſe you ſufficiently inquiſitive.

And though I pretend not to acquaint you, on this occaſion, with any ſtore of new Diſcoveries, yet poſſibly I ſhall be ſo happy, as to aſſiſt you to *know* ſomethings which you did formerly but *ſuppoſe*; and ſhall preſent you, if not with new Theories, at leaſt with new *Proofs* of ſuch as are not yet become unqueſtionable. And if what I ſhall deliver, have the good fortune to encourage and aſſiſt you to proſecute the Hints it will afford, I ſhall account

count my ſelf, in paying of a duty to you, to have done a piece of Service to the Commonwealth of Learning. Since it may highly conduce to the advancement of that Experimental Philoſophy, the effectual purſuit of which, requires as well a Purſe as a Brain, to endeere it to *hopeful* Perſons of your Quality: who may accompliſh many things which others can but *wiſh* or, at moſt, but *deſign*, by being able to imploy the Preſents of Fortune in the ſearch of the Myſteries of Nature.

And I am not faintly induc'd to make choice of this Subject, rather then any of the expected Chymical ones, to entertain your Lordſhip upon, by theſe two Conſiderations: The one, That the Ayr being ſo neceſſary to humane Life, that not onely the generality of Men, but moſt other Creatures that breath, cannot live many *minutes* without it; any conſiderable diſcovery of its Nature, ſeems likely to prove of moment to Man-kinde. And the other is, That the Ambient Ayr, being that whereto both our own Bodies, and moſt of the others we deal with here below, are almoſt perpetually contiguous; not onely its alte-

rations have a notable and manifest share in those obvious effects, that men have already been invited to ascribe thereunto such as are the various distempers incident to humane Bodies, especially if crazy, in the Spring, the Autumn, and also on most of the great and sudden changes of Weather) but likewise, that the further discovery of the nature of the Ayr, will probably discover to us, that it concurs more or less to the exhibiting of many *Phænomena*, in which it hath hitherto scarce been suspected to have any interest. So that a True Account of any Experiment that is New concerning a thing, wherewith we have such constant and necessary intercourse, may not onely prove of some advantage to humane Life, but gratifie Philosophers, by promoting their Speculations on a Subject which hath so much opportunity to sollicite their Curiosity.

And I should immediately proceed to the mention of my Experiments, but that I like too well that worthy saying of the Naturalist *Pliny*, *Benignum est & plenum ingenui pudoris, fateri per quos profeceris*, not to conform to it, by acquainting your Lordship

In Præfat. lib. 1.

ship, in the first place, with the Hint I had of the Engine I am to entertain you of. You may be pleas'd to remember, that a while before our separation in *England*, I told you of a Book that I had heard of, but not perus'd, publish'd by the industrious Jesuit *Schottus*, wherein 'twas said, He related how that ingenious Gentleman *Otto Gericke*, Consul of *Magdeburg*, had lately practiced in *Germany* a way of emptying Glass Vessels, by sucking out the Ayr at the mouth of the Vessel, plung'd under water: And you may also perhaps remember, that I express'd my self much delighted with this Experiment, since thereby the great force of the external Air (either rushing in at the open'd Orifice of the empty'd Vessel, or violently forcing up the Water into it) was rendred more obvious and conspicuous, than in any Experiment that I had formerly seen. And though it may appear by some of those Writings I sometimes shew'd your Lordship, that I had been sollicitous to try things upon the same ground; yet in regard this Gentleman was before-hand with me in producing such considerable effects, by means of the exsuction of Air, I think my self oblig'd

to acknowledge the Aſſiſtance, and Encouragement the Report of his performances hath afforded me.

But as few inventions happen to be at firſt ſo compleat, as not to be either blemiſhd with ſome deficiencies needful to be remedy'd, or otherwiſe capable of improvement: ſo when the Engine we have been ſpeaking of, comes to be more attentively conſider'd, there will appear two very conſiderable things to be deſir'd in it. For firſt, the *Wind-Pump* (as ſome body not improperly calls it) is ſo contriv'd, that to evacuate the Veſſel there is requir'd the continual labor of two ſtrong men for divers hours. And next (which is an imperfection of much greater moment) the Receiver, or Glaſs to be empty'd, conſiſting of one entire and uninterrupted Globe and Neck of Glaſs; the whole Engine is ſo made, that things cannot be convey'd into it, whereon to try Experiments: So that there ſeems but little (if any thing) more to be expected from it, then thoſe very few *Phænomena* that have been already obſerv'd by the Author, and Recorded by *Schottus*. Wherefore to remedy theſe Inconveniences; I put both Mr. *G.*

and *R. Hook* (who hath alſo the Honor to be known to your Lordſhip, and was with me when I had theſe things under conſideration) to contrive ſome Air Pump, that might not, like the other, need to be kept under water (which on divers occaſions is inconvenient) & might be more eaſily manag'd: And after an unſucceſsful tryall or two of ways propoſ'd by others, the laſt nam'd Perſon fitted me with a Pump, anon to be deſcrib'd. And thus the firſt Imperfection of the *German* Engine, was in good meaſure, though not perfectly, remedy'd: And to ſupply the ſecond defect, it was conſidered that it would not perhaps prove impoſſible to leave in the Glaſs to be empty'd, a hole large enough to put in a Mans Arm cloath'd; and conſequently other Bodies, not bigger then it, or longer then the inſide of the Veſſel. And this Deſign ſeem'd the more hopefull, becauſe I remembred, that having ſeveral years before often made the Experiment *De Vacuo* with my own hands; I had, to examine ſome conjectures that occurr'd to me about it, cauſed Glaſſes to be made with a hole at that end, which uſes to be ſeal'd up, and had nevertheleſs been able,

as occasion requir'd, to make use of such Tubes, as if no such holes had been left in them ; by devising stopples for them, made of the common Plaister call'd *Diachylon:* which I rightly enough ghess'd, would, by reason of the exquisite commixtion of its small parts, and closeness of its texture, deny all access to the external Air. Wherefore, supposing that by the help of such Plaisters, carefully laid upon the commissures of the stopple and hole to be made in the Receiver, the external Air might be hindred from insinuating it self between them into the Vessel, we caus'd several such Glasses, as you will finde describ'd a little lower, to be blown at the Glass-house; and though we could not get the Work-men to blow any of them so large, or of so convenient a shape as we would fain have had ; yet finding one to be tolerably fit, and less unfit then any of the rest, we were content to make use of it in that Engine: Of which, I suppose, you by this time expect the Description, in order to the Recital of the *Phænomena* exhibited by it.

To give your Lordship then, in the first place, some account of the Engine it self:

ſelf: It conſiſts of two principal parts; a glaſs Veſſel, and a Pump to draw the Air out of it.

The former of theſe (which we, with the Glaſs-men, ſhall often call a Receiver, for its affinity to the large Veſſels of that name, uſed by Chymiſts) conſiſts of a Glaſs with a wide hole at the top, of a cover to that hole, and of a ſtop-cock faſtned to the end of the neck, at the bottom.

The ſhape of the Glaſs, you will find expreſſ'd in the firſt Figure of the annexed Scheme. And for the ſize of it, it contain'd about 30 Wine Quarts, each of them containing near two pound (of 16 Ounces to the pound) of water: We ſhould have been better pleaſ'd with a more capacious Veſſel, but the Glaſs-men profeſſed themſelves unable to blow a larger, of ſuch a thickneſs and ſhape as was requiſite to our purpoſe.

At the very top of the Veſſel, (A) you may obſerve a round hole, whoſe Diameter (B C) is of about four inches; and whereof, the Orifice is incircled with a lip of Glaſs, almoſt an inch high: For the making of which lip, it was requiſite (to mention that upon the by, in caſe

your

your Lordſhip ſhould have ſuch another Engine made for you) to have a hollow and tapering Pipe of Glaſs drawn out, whereof the Orifice above mentioned was the Baſis, and then to have the cone cut off with a hot Iron, within about an Inch of the Points (B C.)

The uſe of the lip, is to ſuſtain the cover delineated in the ſecond Figure; where (D E) points out a braſs Ring, ſo caſt, as that it doth within and without cover the lip (B C) of the firſt Figure, and is cemented on upon it with a ſtrong and cloſe Cement. To the inward tapering Orifice of this Ring (which is about three Inches over) are exquiſitely ground the ſides of the Braſs ſtopple (F G;) ſo that the concave ſuperficies of the one, and the convex of the other, may touch one another in ſo many places, as may leave as little acceſs, as poſſible, to the external Air: And in the midſt of this cover is left a hole (H I) of about half an inch over, invironed alſo with a ring or ſocket of the ſame mettal, and fitted likewiſe with a braſs ſtopple (K) made in the form of the Key of a ſtop-cock, and exactly ground into the hole (H I) it is to fill; ſo as that though it be turn'd round in the

cavity

cavity it poſſeſſes, it will not let in the Air, and yet may be put in or taken out at pleaſure, for uſes to be hereafter mentioned. In order to ſome of which, it is perforated with a little hole, (8) traverſing the whole thickneſs of it at the lower end; through which, and a little braſs Ring (L) faſtned to one ſide, (no matter which) of the bottom of the ſtopple (F G) a ſtring (8, 9, 10) might paſs, to be imploy'd to move ſome things in the capacity of the empty'd Veſſel; without any where unſtopping it.

The laſt thing belonging to our Receiver, is the ſtop-cock deſigned in the firſt Figure by (N.) for the better faſtening of which to the neck, and exacter excluſion of the Air, there was ſoder'd on to the ſhank of the Cock (X) a Plate of Tin, (M T U W) long enough to cover the neck of the Receiver. But becauſe the cementing of this was a matter of ſome difficulty, it will not be amiſs to mention here the manner of it, which was, That the cavity of the tin Plate was fill'd with a melted Cement, made of Pitch, Roſin, and Wood-aſhes, well incorporated; and to hinder this liquid Mixture from getting into the Orifice (Z)

of

of the ſhank, (X) that hole was ſtopt with a Cork, to which was faſtned a ſtring, whereby it might be pull'd out of the upper Orifice of the Receiver; and then, the glaſs neck of the Receiver being well warm'd, was thruſt into this Cement, and over the ſhank whereby it was effected, that all the ſpace betwixt the tin Plate and the Receiver, and betwixt the internal ſuperficies of the Receiver, and the ſhanck of the Cock, was filld with the Cement; and ſo we have diſpach'd the firſt and upper part of the Engine.

The undermoſt remaining part conſiſts of a Frame, and of a ſucking Pump, or as we formerly call'd it, an Air Pump, ſupported by it: The Frame is of Wood, ſmall, but very ſtrong, conſiſting of three legs, (1 1 1) ſo plac'd, that one ſide of it may ſtand perpendicular, that the free motion of the hand may not be hindered. In the midſt of which frame, is tranſverſly nail'd a board, (2 2 2) which may not improperly be call'd a Midriff, upon which reſts, and to which is ſtrongly faſtned, the main part of the Pump it ſelf, which is the onely thing remaining to be deſcribed.

The Pump conſiſts of four parts, a hollow

hollow Cylindre, a Sucker, a handle to move that Sucker, and a valve.

The Cylindre was (by a pattern) cast of brass; it is in length about 14 inches, thick enough to be very strong, notwithstanding the Cylindrical cavity left within it; this cavity is about three inches Diameter, and makes as exact a Cylindre as the Artificer was able to bore. This hollow Cylindre is fitted with a sucker, (4455) consisting of two parts, the one (44) somewhat less in Diameter then the cavity of the Cylindre, upon which is nail'd a good thick piece of tan'd shoe Leather, which will go so close to the Cylindre, that it will need to be very forcibly knock'd and ram'd in, if at any time it be taken out, which is therefore done, that it may the more exactly hinder the Air from insinuating it self betwixt it and the sides of the Cylindre whereon it is to move.

To the midst of this former part of the Sucker is strongly fastned the other, namely a thick and narrow plate of Iron, (55) somewhat longer then the Cylindre, one of whose edges is smooth, but at the other edge it is indented (as I may so speak) with a row of teeth delineated in

the

the Scheme, into whose intervals are to be fitted, the teeth of a small Iron nut, ($\alpha\beta$) (as Trades-men call it) which is fast-ned by two staples (22) to the under side of the formerly mention'd transverse board (222) on which the Cylindre rests, and is turn'd to and fro by the third piece of this Pump, namely, the handle or *manubrium*, (7) of which the Figure gives a sufficient description.

The fourth and last part of this Cylindre, is the Valve, (R) consisting of a hole bored through at the top of the Cylindre, a little tapering towards the cavity; into which hole is ground a tapering Peg of brass, to be thrust in, and taken out at pleasure.

The Engine being thus describ'd, it will be requisite to adde, that something is wont to be done before it be set on work, for the more easie moving of the Sucker, and for the better exclusion of the outward Air: which when the Vessel begins to be exhausted, is much more difficult to be kept out then one would easily imagine.

There must then be first powr'd in at the top of the Receiver a little sallad oyl, partly to fill up any small intervalls that

may

may happen to be betwixt the contiguous surfaces of the internal parts of the Stop-cock: And partly that it may be the more easie to turn the Key (S) backwards and forwards. Pretty store of oyl must also be pour'd into the Cylindre, both that the Sucker may slip up and down in it the more smoothly and freely, and that the Air might be the better hindred from getting in between them: And for the like reasons, a little oyl is to be used also about the Valve. Upon which occasion, it would not be omitted (for it is strange) that oftentimes, when neither the pouring in of water, nor even of oyl alone, prov'd capable to make the Sucker move easily enough in the Cylinder; a mixture of both those Liquors would readily (sometimes even to admiration) perform the desired effect. And lastly, the brass cover of the Receiver, being put into the brass ring formerly describ'd, that no Air may get between them, it will be very requisite to plaister over very carefully the upper edges of both, with the plaister formerly mentioned, or some other as close, which is to be spread upon the edges with a hot Iron; that being melted, it may run into and

fill up all the crannies, or other little cavities, at which the Air might otherwise get entrance.

All things being thus fitted, and the lower shank (O) of the stop-cock being put into the upper Orifice of the Cylinder(&), into which it was exactly ground; the Experimenter is first, by turning the handle, to force the Sucker to the top of the Cylinder, that there may be no Air left in the upper part of it, Then shutting the Valve with the Plug, and turning the other way, he is to draw down the Sucker to the bottom of the Cylinder; by which motion of the Sucker, the Air that was formerly in the Cylinder being thrust out, and none being permitted to succeed in its room, 'tis manifest that the cavity of the Cylinder must be empty, in reference to the Air. So that if thereupon the Key of the Stop-cock be so turn'd, as that through the perforation of it, a free passage be opened betwixt the Cylinder and the Receiver, part of the Air formerly contain'd in the Receiver, will nimbly descend into the Cylinder. And this Air, being by the turning back of the Key hinder'd from the returning into the Receiver, may, by the opening

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he Valve, and forcing up of the Suck- o the top of the Cylinder again, be en out into the open Air. And thus the repetition of the motion of the ker upward and downward, and by op- unely turning the Key, and ſtopping Valve, as occaſion requires, more or Air may be ſuck'd out of the Recei- ccording to the exigency of the Ex- ent, and the intention of him that s it.

ur Lordſhip will, perhaps, think that e been unneceſſarily prolix in this art of my Diſcourſe: But if you en how many unexpected difficul- ve found to keep out the externall even for a little while, when ſome derable part of the internal had been out; You would peradventure al- that I might have ſet down more nſtances then I have, without ſet- own any, whoſe knowledge, he that ry the Experiment may not have of. Which is ſo true, that, before we ed any further, I cannot think it un- able to advertiſe Your Lordſhip, ere are two chief ſorts of Experi- , which we deſign'd in our Engine ke tryal of: The one, ſuch as may

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of the Valve, and forcing up of the Sucker to the top of the Cylinder again, be driven out into the open Air. And thus by the repetition of the motion of the Sucker upward and downward, and by opportunely turning the Key, and ſtopping the Valve, as occaſion requires, more or leſs Air may be ſuck'd out of the Receiver, according to the exigency of the Experiment, and the intention of him that makes it.

Your Lordſhip will, perhaps, think that I have been unneceſſarily prolix in this firſt part of my Diſcourſe: But if you had ſeen how many unexpected difficulties we found to keep out the externall Air, even for a little while, when ſome conſiderable part of the internal had been ſuckt out; You would peradventure allow, that I might have ſet down more circumſtances then I have, without ſetting down any, whoſe knowledge, he that ſhall try the Experiment may not have need of. Which is ſo true, that, before we proceed any further, I cannot think it unſeaſonable to advertiſe Your Lordſhip, that there are two chief ſorts of Experiments, which we deſign'd in our Engine to make tryal of: The one, ſuch as may

be quickly dispatcht, and therefore may be try'd in our Engine, though it leak a little; because the Air may be faster drawn out, by nimbly plying the Pump, then it can get in at undiscern'd leaks; I say at undiscern'd leaks, because such as are big enough to be discover'd can scarce be uneasie to be stopt. The other sort of Experiments consists of those that require not onely that the internal Air be drawn out of the Receiver, but that it be likewise for a long time kept out of it. Such are the preservation of Animal and other Bodies therein, the germination and growth of Vegetables, and other tryals of several sorts, which it is apparent cannot be well made unless the external Air can, for a competent while, be excluded: Since even at a very small leak there may enough get in, to make the *Vacuum* soon loose that name; by which I here declare once for all, that I understand not a space wherein there is no body at all, but such as is either altogether, or almost totally void of Air.

Now this distinction of Experiments I thought fit to premise to the ensuing Narratives, because upon tryal, we found it so exceeding (and scarce imaginable) difficult

ficult a matter, to keep out the Air from getting at all in at any imperceptible hole or flaw whatsoever, in a vessel immediately surrounded with the compressed Atmosphere, that in spight of all our care and diligence, we never were able totally to exhaust the Receiver, or keep it when it was almost empty, any considerable time, from leaking more or less: although (as we have lately intimated) by unwearyed quickness in plying the Pump, the internall Air can be much faster drawn out then the external can get in, till the Receiver come to be almost quite empty. And that's enough to enable men to discover hitherto unobserved *Phænomena* of Nature.

The Experiments therefore of the first sort, will, I fear, prove the onely ones wherewith my Avocations will allow me to entertain Your Lordship in this Letter. For till your further Commands shall engage me to undertake, by Gods permission, such an Employment, and more leasure shall better fit me for it, I know not whether I shall be in a condition to try what may be done, to enable me to give you some account of the other sort of Experiments also.

Experiment I.

TO proceed now to the *Phænomena*, exhibited to us by the Engine above described; I hold it not unfit to begin with what does constantly and regularly offer it self to our observation, as depending upon the Fabrick of the Engine it self, and not upon the nature of this or that particular Experiment which 'tis employed to try.

First, Then upon the drawing down of the Sucker, (the Valve being shut) the Cylindrical space, deserted by the Sucker, is left devoid of Air; and therefore, upon the turning of the Key, the Air contained in the Receiver rushes into the emptyed Cylinder, till the Air in both those Vessels be brought to about an equal measure of dilatation. And therefore, upon shutting the Receiver by returning the Key, if you open the Valve, and force up the Sucker again, you will finde, that after this first exsuction you will drive out almost a whole Cylinder full of Air: But at the following exsuctions, you will draw less and less of Air out of the Receiver into the Cylinder, because that there will still remain less and less Air in the

Receiver

Receiver it ſelf; and conſequently, the Particles of the remaining Air, having more room to extend themſelves in, will leſs preſs out one another. This you will eaſily perceive, by finding, that you ſtill force leſs and leſs Air out of the Cylinder; ſo that when the Receiver is almoſt exhauſted, you may force up the Sucker almoſt to the top of the Cylinder, before you will need to unſtop the Valve to let out any Air: And if at ſuch time, the Valve being ſhut, you let go the handle of the Pump, you will finde the Sucker forcibly carryed up to the top of the Cylinder, by the protruſion of the external Air; which, being much leſs rarified then that within the Cylinder, muſt have a more forcible preſſure upon the Sucker, then the internal is able to reſiſt: And by this means you may know how far you have emptyed the Receiver. And to this we may adde, on this occaſion, that conſtantly upon the turning of the Key to let out the Air from the Receiver, into the emptied Cylinder, there is immediately produced a conſiderably brisk noiſe, eſpecially whil'ſt there is any plenty of Air in the Receiver.

For the more eaſie underſtanding of the Experiments tryable by our Engine, I thought it not ſuperfluous, nor unſeaſonable in the recital of this firſt of them, to inſinuate that notion by which it ſeems likely that moſt, if not all, of them will prove explicable. Your Lordſhip will eaſily ſuppoſe, that the Notion I ſpeak of is, That there is a Spring, or Elaſtical power in the Air we live in. By which ἐλατὴρ or Spring of the Air; that which I mean is this: That our Air either conſiſts of, or at leaſt abounds with, parts of ſuch a nature, that in caſe they be bent or compreſſ'd by the weight of the incumbent part of the Atmoſphere, or by any other Body, they do endeavor, as much as in them lies, to free themſelves from that preſſure, by bearing againſt the contiguous Bodies that keep them bent; and, aſſoon as thoſe Bodies are remov'd or reduced to give them way, by preſently unbending and ſtretching out themſelves, either quite, or ſo far forth as the contiguous Bodies that reſiſt them will permit, and thereby expanding the whole parcel of Air, theſe elaſtical Bodies compoſe.

This Notion may perhaps be somewhat further explain'd, by conceiving the Air near the Earth to be such a heap of little Bodies, lying one upon another, as may be resembled to a Fleece of Wooll. For this (to omit other likenesses betwixt them) consists of many slender and flexible Hairs; each of which, may indeed, like a little Spring, be easily bent or rouled up; but will also, like a Spring, be still endeavouring to stretch it self out again. For though both these Haires, and the Aerial Corpuscles to which we liken them, do easily yield to externall pressures; yet each of them (by vertue of its structure) is endow'd with a Power or Principle of self-Dilatation; by vertue whereof, though the hairs may by a Mans hand be bent and crouded closer together, and into a narrower room then suits best with the nature of the Body: Yet whil'st the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposes its Expansion. And upon the removall of the external pressure, by opening the hand more or less, the compressed Wooll does, as it were, spontaneously expand or display it self towards

the recovery of its former more looſe and free condition, till the Fleece have either regain'd its former Dimenſions, or at leaſt, approach'd them as near as the compreſſing hand (perchance not quite open'd) will permit. This Power of ſelf-Dilatation, is ſomewhat more conſpicuous in a dry Spunge compreſſ'd, then in a Fleece of Wooll. But yet we rather choſe to imploy the latter, on this occaſion, becauſe it is not like a Spunge, an entire Body, but a number of ſlender and flexible Bodies, looſely complicated, as the Air it ſelf ſeems to be.

There is yet another way to explicate the Spring of the Air, namely, by ſuppoſing with that moſt ingenious Gentleman, Monſieur *Des Cartes*, That the Air is nothing but a Congeries or heap of ſmall and (for the moſt part) of flexible Particles; of ſeveral ſizes, and of all kinde of Figures which are raiſ'd by heat (eſpecially that of the Sun) into that fluid and ſubtle Etheriall Body that ſurrounds the Earth; and by the reſtleſſe agitation of that Celeſtial Matter wherein thoſe Particles ſwim, are ſo whirl'd

round,

round, that each Corpuſcle endeavours to beat off all others from coming within the little Sphear requiſite to its motion about its own Center; and (in caſe any, by intruding into that Sphear ſhall oppoſe its free Rotation) to expell or drive it away: So that according to this Doctrine, it imports very little, whether the particles of the Air have the ſtructure requiſite to Springs, or be of any other form (how irregular ſoever) ſince their Elaſtical power is not made to depend upon their ſhape or ſtructure, but upon the vehement agitation, and (as it were) brandiſhing motion, which they receive from the fluid *Ether* that ſwiftly flows between them, and whirling about each of them (independently from the reſt) not onely keeps thoſe ſlender Aërial Bodies ſeparated and ſtretcht out (at leaſt, as far as the Neighbouring ones will permit) which otherwiſe, by reaſon of their flexibleneſs and weight, would flag or curl; but alſo makes them hit againſt, and knock away each other, and conſequently require more room, then that which if they were compreſſ'd, they would take up.

By

By these two differing ways, my Lord, may the Spring of the Air be explicated. But though the former of them be that, which by reason of its seeming somewhat more easie, I shall for the most part make use of in the following Discourse: yet am I not willing to declare peremptorily for either of them, against the other. And indeed, though I have in another Treatise endeavoured to make it probable, that the returning of Elastical Bodies (if I may so call them) forcibly bent, to their former position, may be Mechanically explicated: Yet I must confess, that to determine whether the motion of Restitution in Bodies, proceed from this, That the parts of a Body of a peculiar Structure are put into motion by the bending of the spring, or from the endeavor of some subtle ambient Body, whose passage may be oppos'd or obstructed, or else it's pressure unequally resisted by reason of the new shape or magnitude, which the bending of a Spring may give the Pores of it: To determine this, I say, seems to me a matter of more difficulty, then at first sight one would easily imagine it. Wherefore I shall decline medling with a subject, which is much more hard to be explica-

ted, then necessary to be so, by him, whose business it is not, in this Letter, to assign the adequate cause of the Spring of the Air, but onely to manifest, That the Air has a Spring, and to relate some of its effects.

I know not whether I need annex that, though either of the above-mention'd Hypotheses, and perhaps some others, may afford us an account plausible enough of the Air-spring; yet I doubt, whether any of them gives us a sufficient account of its Nature. And of this doubt, I might here mention some Reasons, but that, peradventure, I may (God permitting) have a fitter occasion to say something of it elsewhere. And therefore I should now proceed to the next Experiment, but that I think it requisite, first, to suggest to your Lordship what comes into my thoughts, by way of Answer to a plausible Objection, which I foresee you may make against our propos'd Doctrine, touching the Spring of the Air. For it may be alleadged, that though the Air were granted to consist of Springy Particles (if I may so speak) yet thereby we could onely give an account of the Dilatation of the Air in Wine-Guns and

other pneumatical Engines wherein the Air has been compress'd, and its Springs violently bent by an apparent externall force; upon the removall of which, 'tis no wonder that the Air should, by the motion of restitution, expand it self till it have recovered its more natural dimensions: whereas in our above mentioned first Experiment, and in almost all others tryable in our Engine, it appears not that any compression of the Air precededits spontaneous Dilatation or Expansion of it self. To remove this difficulty, I must desire Your Lordship to take notice, that of whatever nature the Air, very remote from the Earth, may be, and whatever the Schools may confidently teach to the contrary, yet we have divers Experiments to evince, that the Atmosphere we live in is not (otherwise then comparatively to more ponderous Bodies) light, but heavy: And did not their gravity hinder them, it appears not why the steams of the Terraqueous Globe, of which our Air in great part consists, should not rise much higher then the Refraction of the Sun, and other Stars give men ground to think, that the Atmosphere, even in the judgement of those

Recent Aſtronomers, who ſeem willing to enlarge its bounds as much as they dare, does reach.

But leſt you ſhould expect my ſeconding this Reaſon by Experience; and leſt you ſhould object, That moſt of the Experimēts that have been propoſ'd to prove the gravity of the Air, have been either barely propoſ'd, or perhaps not accuratly try'd; I am content, before I paſs further, to mention here, That I found a dry lambs-bladder containing near about two thirds of a pint, and compreſſ'd by a packthred tyed about it, to looſe a grain and the eighth part of a grain of its former weight, by the receſs of the Air upon my having prickt it: And this with a pair of Scales, which when the full Bladder and the correſpondent weight were in it, would manifeſtly turn either way with the 32 part of a grain. And if it be further objected, That the Air in the Bladder was violently compreſſ'd by the Pack-thred and the ſides of the Bladder, we might probably (to wave prolix anſwers) be furniſh'd with a Reply, by ſetting down the differing weight of our Receiver, when empty'd and when full of uncompreſſ'd Air, if we could here procure ſcales fit for ſo nice an experiment; ſince

ſince we are informed, that in the *German* Experiment, commended at the beginning of this Letter, the Ingenious Tryers of it found, That their Glaſs Veſſel, of the capacity of 32 meaſures, was lighter when the Air had been drawn out of it, then before, by no leſs then one ounce and $\frac{3}{10}$ that is, an ounce and very near a third: But of the gravity of the Air, we may elſewhere have occaſion to make further mention.

Taking it then for granted that the Air is not devoid of weight, it will not be uneaſie to conceive, that that part of the Atmoſphere wherein we live, being the lower part of it, the Corpuſcles that compoſe it, are very much compreſs'd by the weight of all thoſe of the like nature that are directly over them, that is, of all the Particles of Air, that being pil'd up upon them, reach to the top of the Atmoſphere. And though the height of this Atmoſphere, according to the famous *Kepler*, and ſome others, ſcarce exceeds eight common miles; yet other eminent and later Aſtronomers, would promote the confines of the Atmoſphere, to exceed ſix or ſeven times that number of miles. And the diligent and learned

Riviolo

Riviolo makes it probable, that the Atmoſphere may, at leaſt in divers places, be at leaſt 50 miles high. So that according to a moderate eſtimate of the thickneſs of the Atmoſphere, we may well ſuppoſe, that a Column of Air, of many miles in height, leaning upon ſome ſpringy Corpuſcles of Air here below, may have weight enough to bend their little ſprings, and keep them bent: As, to reſume our former compariſon, if there were fleeces of Wooll pil'd up to a mountainous height upon one another, the Hairs that compoſe the lowermoſt locks which ſupport the reſt, would, by the weight of all the Wool above them, be as well ſtrongly compreſſed, as if a man ſhould ſqueeze them together in his hands, or imploy any ſuch other moderate force to compreſs them. So that we need not wonder, that upon the taking off the incumbent Air from any parcel of the Atmoſphere here below, the Corpuſcles, whereof that undermoſt Air conſiſts, ſhould diſplay themſelves, and take up more room then before.

And if it be objected, That in Water, the weight of the upper and of the lower part is the ſame: I anſwer, That beſides

that,

that it may be well doubted whether the obſervation, by reaſon of the great difficulty have been exactly made, there is a manifeſt diſparity betwixt the Air and Water: For I have not found, that upon an Experiment purpoſely made, (and in another Treatiſe Recorded) that Water will ſuffer any conſiderable compreſſion; whereas we may obſerve in Wind-Guns (to mention now no other Engines) that the Air will ſuffer it ſelf to be crouded into a comparatively very little room; in ſo much, that a very diligent Examiner of the *Phænomena* of Wind-Guns would have us believe, that in one of them, by condenſation, he reduc'd the Air into a ſpace at leaſt eight times narrower then it before poſſeſt. And to this, if we adde a noble *Phænomenon* of the Experiment *De Vacuo*; theſe things put together, may for the preſent ſuffice to countenance our Doctrine. For that noble Experimenter, Monſieur *Paſcal* (the Son) had the commendable Curioſity to cauſe the *Torricellian* Experiment to be try'd at the foot, about the middle, and at the top of that high Mountain (in *Auvergne*, if I miſtake not) commonly call'd *Le Puy de Domme*; whereby it was found, That the *Mercury*

in

in the Tube fell down lower, about three inches, at the top of the Mountain then at the bottom. And a Learned Man a while since inform'd me, That a great *Virtuoso*, friend to us both, has, with not unlike success, tryed the same Experiment in the lower and upper parts of a Mountain in the West of *England*: Of which, the reason seems manifestly enough to be this, That upon the tops of high Mountains, the Air which bears against the restagnant Quick-silver, is less press'd by the less ponderous incumbent Air; and consequently is not able totally to hinder the descent of so tall and heavy a Cylinder of Quick-silver, as at the bottom of such Mountains did but maintain an *Æquilibrium* with the incumbent Atmosphere.

And if it be yet further Objected against what hath been propos'd touching the compactness and pressure of the Inferior Air; That we finde this very Air to yield readily to the motion of little Flies, and even to that of Feathers, and such other light and weak Bodies; which seems to argue, that the particles of our Air are not so compress'd as we have represented them, especially, since by our former Experiment it appears, that the Air rea-

dily dilated it ſelf downward, from the Receiver into the Pump, when 'tis plain, that it is not the incumbent Atmoſphere, but onely the ſubjacent Air in the braſs Cylinder that has been remov'd: If this, I ſay, be objected, we may reply, That when a man ſqueezes a Fleece of Wool in his hand, he may feel that the Wool inceſſantly bears againſt his hand, as that which hinders the hairs it conſiſts of, to recover their former and more natural extent. So each parcel of the Air about the Earth, does conſtantly endeavour to thruſt away all thoſe contiguous Bodies, whether Aërial or more groſs, that keep them bent, and hinder the expanſion of its parts, which will dilate themſelves or flie abroad towards that part, whether upwards or downwards, where they finde their attempted Dilatation of themſelves leſs reſiſted by the neihgboring Bodies. Thus the Corpuſcles of that Air we have been all this while ſpeaking of, being unable, by reaſon of their weight, to aſcend above the Convexity of the Atmoſphere, and by reaſon of the reſiſtance of the ſurface of the Earth and Water, to fall down lower, they are forced, by their own gravity and this reſiſtance, to expand and

diffuſe

diffuse themselves about the Terrestial Globe; whereby it comes to pass, that they must as well press the contiguous Corpuscles of Air that on either side oppose their Dilatation, as they must press upon the surface of the Earth, and, as it were recoyling thence, endeavor to thrust away those upper particles of Air that lean upon them.

And as for the easie yielding of the Air to the Bodies that move in it, if we consider that the Corpuscles whereof it consists, though of a springy nature, are yet so very small, as to make up (which 'tis manifest they doe) a fluid Body, it will not be difficult to conceive, that in the Air, as in other Bodies that are fluid, the little Bodies it consists of are in an almost restless motion, whereby they become (as we have more fully discoursed in another Treatise) very much disposed to yield to other Bodies, or easie to be displac'd by them, and that the same Corpuscles are likewise so variously mov'd, as they are intire Corpuscles, that if some strive to push a Body plac'd among them towards the right hand (for instance) others, whose motion has an opposite determination, as strongly thrust the same

In a Discourse touching fluidity and firmness.

Body towards the left; whereby neither of them proves able to move it out of its place, the pressure on all hands being reduced as it were to an *Æquilibrium*: so that the Corpuscles of the Air must be as well sometimes considered under the notion of little Springs, which remaining bent, are in their entire bulk transported from place to place; as under the notion of Springs displaying themselves, whose parts flie abroad whilst as to their entire bulk they scarce change place: As the two ends of a Bow, shot off, fly from one another, whereas the Bow it self may be held fast in the Archers hand; and that it is the equal pressure of the Air on all sides upon the Bodies that are in it, which causes the easie Cession of its parts, may be argu'd from hence: That if by the help of our Engine the Air be but in great part, though not totally drawn away from one side of a Body without being drawn away from the other; he that shall think to move that Body too and fro, as easily as before, will finde himself much mistaken.

In verification of which we will, to divert your Lordship a little, mention here a *Phænomenon* of our Engine, which even

to divers ingenious perſons has at firſt ſight ſeem'd very wonderful.

THe thing that is wont to be admired, and which may paſs for our ſecond Experiment is this, That if, when the Receiver is almoſt empty, a By-ſtander be deſired to lift up the braſs Key (formerly deſcribed as a ſtopple in the braſs Cover) he will finde it a very difficult thing to do ſo, if the Veſſel be well exhauſted; and even when but a moderate quantity of Air has been drawn out, he will, when he has lifted it up a little, ſo that it is ſomewhat looſe from the ſides of the lip or ſocket, which (with the help of a little oyl) it exactly filled before, he will (I ſay) finde it ſo difficult to be lifted up, that he will imagine there is ſome great weight faſtned to the bottom of it. And if (as ſometimes has been done for merriment) onely a Bladder be tyed to it, it is pleaſant to ſee how men will marvail that ſo light a Body, filled at moſt but with Air, ſhould ſo forcibly draw down their hand as if it were fill'd with ſome very ponderous thing: whereas the cauſe of this pretty *Phænomenon* ſeems plainly enough to *Experiment 2.*

 be

be onely this, That the Air in the Receiver, being very much dilated, its Spring must be very much weakn'd, and consequently it can but faintly press up the lower end of the stopple, whereas the Spring of the external Air being no way debilitated, he that a little lifts up the stopple must with his hand support a pressure equal to the disproportion betwixt the force of the internal expanded Air, and that of the Atmosphere incumbent upon the upper part of the same key or stopple: And so men being unus'd to finde any resistance, in lifting things up, from the free Air above them, they are forward to conclude that that which depresses their hands must needs be some weight, though they know not where plac'd, drawing beneath it.

And that we have not mis-assign'd the cause of this *Phænomenon* seems evident enough by this; That as Air is suffered by little and little to get into the Receiver, the weight that a man fancies his hand supports is manifestly felt to decrease more and more, the internal Air by this recruit approaching more to an *Æquilibrium* with the external, till at length the Receiver growing again full of Air, the

ſtopple may be lifted up without any difficulty at all.

By ſeveral other of the Experiments afforded us by our Engine, the ſame notion of the great and equal preſſure of the free Air upon the Bodies it environs, might be here manifeſted, but that we think it not ſo fit to anticipate ſuch Experiments: And therefore ſhall rather employ a few lines to clear up a difficulty touching this matter, which we have obſerv'd to have troubled ſome even of the Philoſophical and Mathematical Spectators of our Engine, who have wonder'd that we ſhould talk of the Air exquiſitely ſhut up in our Receiver, as if it were all one with the preſſure of the Atmoſphere; whereas the thick and cloſe body of the Glaſs, wholly impervious to the Air, does manifeſtly keep the incumbent Pillar of the Atmoſphere from preſſing in the leaſt upon the Air within the Glaſs, which it can no where come to touch. To elucidate a little this matter, let us conſider, That if a man ſhould take a fleece of Wool, and having firſt by compreſſing it in his hand reduc'd it into a narrower compaſs, ſhould nimbly convey and ſhut it cloſe up into a Box juſt fit for it, though

the force of his hand would then no longer bend those numerous springy Body's that compose the Fleece, yet they would continue as strongly bent as before, because the Box they are inclos'd in would as much resist their re-expanding of themselves, as did the hand that put them in. For thus we may conceive, that the Air being shut up, when its parts are bent by the whole weight of the incumbent Atmosphere, though that weight can no longer lean upon it, by reason it is kept off by the Glass, yet the Corpuscles of the Air within that Glass continue as forcibly bent as they were before their inclusion, because the sides of the Glass hinder them from displaying or stretching out themselves. And if it be objected that this is unlikely, because ev'n Glass bubles, such as are wont to be blown at the flame of a Lamp, exceeding thin and Hermetically seal'd will not break; whereas it cannot be imagin'd that so thin a Prison of Glass could resist the Elastical force of all the included Air, if that Air were so compress'd as we suppose. It may be easily reply'd, That the pressure of the inward Air against the Glass, is countervail'd by the equal pres-

ſure of the outward againſt the ſame Glaſs. And we ſee in bubles, that by reaſon of this an exceeding thin film of Water is often able, for a good while, to hinder the eruption of a pretty quantity of Air. And this may be alſo more conſpicuous in thoſe great Spherical bubles that boyes ſometimes blow with Water, to which Sope has given a Tenacity. But that, if the preſſure of the ambient Air were remov'd, the internal Air may be able to break thicker Glaſſes then thoſe lately mention'd, will appear by ſome of the following Experiments; to which we ſhall therefore now haſten, having, I fear, been but too prolix in this Excurſion, though we thought it not amiſs to annex to our firſt Experiments ſome general Conſiderations touching the Spring of the Air, because (this Doctrine being yet a ſtranger to the Schools) not onely we finde not the thing it ſelf to be much taken notice of; but of thoſe few that have heard of it, the greater part have been forward to reject it, upon a miſtaken Perſwaſion, that thoſe *Phænomena* are the effects of natures abhorrency of a *Vacuum*, which ſeem to be more fitly aſcribeable to the weight and Spring of the Air.

We

Experiment 3. WE will now proceed to obſerve that though, by the help of the handle, the Sucker be eaſily drawn down to the bottom of the Cylinder; yet, without the help of that Leaver, there would be required to the ſame effect, a force or weight great enough to ſurmount the preſſure of the whole Atmoſphere: Since otherwiſe the Air would not be driven out of its place, when none is permitted to ſucceed into the place deſerted by the Sucker. This ſeems evident, from the known *Torricellian* Experiment, in which, if the inverted Tube of *Mercury* be but 25 Digits high, or ſomewhat more, the Quick-ſilver will not fall but remain ſuſpended in the Tube; becauſe it cannot preſs the ſubjacent *Mercury* with ſo great a force, as does the incumbent Cylinder of the Air reaching thence to the top of the Atmoſphere: Whereas, if the Cylinder of *Mercury* were three or four digits longer, it would over-power that of the external Air, and run out into the Veſſel'd *Mercury*, till the two Cylinders came to an *Æquilibrium*, and no further. Hence we need not wonder, that though the

Sucker

Sucker move easily enough up and down in the Cylinder by the help of the *Manubrium*; yet if the *Manubrium* be taken off, it will require a considerable strength to move it either way. Nor will it seem strange, that if, when the Valve and Stop-cock are well shut, you draw down the Sucker, and then let go the *Manubrium*; the Sucker will, as it were of it self, re-ascend to the top of the Cylinder, since the spring of the external Air findes nothing to resist its pressing up the Sucker. And for the same reason, when the Receiver is almost evacuated, though, having drawn down the Sucker, you open the way from the Receiver to the Cylinder, and then intercept that way again by returning the Key; the Sucker will, upon the letting go the *Manubrium*, be forcibly carried up almost to the top of the Cylinder: Because the Air within the Cylinder, being equally dilated and weakned with that of the Glass, is unable to withstand the pressure of the external Air, till it be driven into so little space, that there is an *Æquilibrium* betwixt its force and that of the Air without. And congruously hereunto we finde, that in this case, the Sucker is drawn down with little

less difficulty, then if the Cylinder, being devoid of Air, the Stop-cock were exactly shut: We might take notice of some other things, that depend upon the Fabrick of our Engine it self; but to shun prolixity, we will, in this place, content our selves to mention one of them, which seems to be of greater moment then the rest, and it is this; that when the Sucker has been impell'd to the top of the Cylinder, and the Valve is so carefully stopp'd, that there is no Air left in the Cylinder above the Sucker: If then the Sucker be drawn to the lower part of the Cylinder, he that manages the Pump findes not any sensibly greater difficulty to depress the Sucker, when it is nearer the bottom of the Cylinder, then when it is much further off. Which circumstance we therefore think fit to take notice of, because an eminent Modern Naturalist hath taught, that, when the Air is sucked out of a Body, the violence wherewith it is wont to rush into it again, as soon as it is allow'd to re-enter, proceeds mainly from this; That the pressure of the ambient Air is strengthned upon the accession of the Air suck'd out; which, to make it self room, forces the neighboring Air to a violent-subingression of its parts: which, if it were true, he that draws

down

down the Sucker, would finde the resistance of the external Air increas'd as he draws it lower, more of the displaced Air being thrust into it to compress it. But, by what has been discours'd upon the first Experiment, it seems more probable, that without any such strengthning of the pressure of the outward Air, the taking quite away or the debilitating of the resistance from within, may suffice to produce the effects under consideration. But this will perhaps be illustrated by some or other of our future Experiments, and therefore shall be no longer insisted on here.

HAving thus taken-notice of some of the constant *Phænomena* of our Engine it self, let us now proceed to the Experiments tryable in it. *Experiment 4.*

We took then a Lambs Bladder large, well dry'd, and very limber, and leaving in it about half as much Air as it could contain, we caus'd the neck of it to be strongly ty'd, so that none of the included Air, though by pressure, could get out. This Bladder being convey'd into the Receiver, and the Cover luted on; the Pump was set awork, and after two or three exsuctions of the ambient Air (whereby the Spring of that which remain'd in the

the Glaſs was weaken'd) the Impriſon'd Air began to ſwell in the Bladder, and as more and more of the Air in the Receiver was, from time to time, drawn out; ſo did that in the Bladder more and more expand it ſelf, and diſplay the folds of the formerly flaccid Bladder: ſo that before we had exhauſted the Receiver near ſo much as we could, the Bladder appear'd as full and ſtretched, as if it had been blown up with a Quill.

And that it may appear that this plumpneſs of the Bladder proceeded from the ſurmounting of the debilitated Spring of the ambient Air remaining in the Veſſel, by the ſtronger Spring of the Air remaining in the Bladder; we Return'd the Key of the Stop-cock, and by degrees allow'd the external Air to return into the Receiver: Whereupon it happen'd, as was expected, that as the Air came in from without, the diſturb'd Air in the Bladder, was proportionably compreſſ'd into a narrower room, and the ſides of the Bladder grew flaccid, till the Receiver having readmitted its wonted quantity of Air, the Bladder appear'd as full of wrinkles and cavities as before.

This

This Experiment is much of the ſame nature with that which was ſome years agoe ſaid to be made by that eminent Geometrician Monſieur *Roberval*, with a Carps Bladder empty'd and convey'd into a Tube, wherein the Experiment *De Vacuo* was afterwards try'd, which ingenious Experiment of his juſtly deſerves the thanks of thoſe that have been, or ſhall be ſolicitous to diſcover the nature of the Air.

But to return to our Experiment, we may take notice of this Circumſtance in it, That after the Receiver has been in ſome meaſure empty'd, the Bladder do's, at each exſuction, ſwell much more conſpicuouſly then it did at any of the firſt Exſuctions; inſomuch that towards the end of the pumping, not onely a great fold or cavity in the ſurface of the Bladder may be made, even by the ſtretching of the inward ſelf-expanding Air: But we have ſometimes ſeen, upon the turning of the Key to let the ambient Air paſs out of the Receiver into the Cylinder, we have ſeen (I ſay) the Air in the Bladder ſuddenly expand it ſelf ſo much and ſo briskly, that it manifeſtly lifted up ſome light Bodies that lean'd upon it,

and ſeem'd to lift up the Bladder it ſelf.

Now becauſe it has by very Learned Men been doubted whether the ſwelling of the Bladder may not have proceeded from the Dilatation of the included Air, but from the Texture of the Fibres, which, being wont to keep the Bladder extended when the Animal to whom it belong'd was alive, may be ſuppos'd in our Experiment to have return'd, like ſo many Springs to their wonted extent, upon the removal of the Ambient Air that compreſs'd and bent them: becauſe this, I ſay, has been doubted, we thought fit to make this further tryall.

We let down into the Receiver with the fore-mentioned Bladder two other much ſmaller, and of the ſame kinde of Animal; the one of theſe was not ty'd up at the neck that there might be liberty left to the Air that was not ſqueez'd out (which might amount to about a fifth part of what the Bladder held before) to paſs out into the Receiver: The other had the ſides of it ſtretch'd out and preſs'd together, almoſt into the form of a Cup, that they might intercept the leſs Air betwixt them, and then was ſtrongly ty'd

up

up at the neck: This done, and the Air being in ſome meaſure ſuck'd out of the Pneumatical Glaſs (if I may ſo call it) the Bladder, mention'd at the beginning of our Experiment, appear'd extended every way to its full Dimenſions; whereas neither of the two others did remarkably ſwell, and that whoſe neck was not ty'd ſeem'd very little, if at all leſs wrinkl'd then when it was put in.

We made likewiſe a ſtrong Ligature about the middle of a long Bladder partly empty'd, and upon the drawing the Air out of the Receiver, could obſerve no ſuch ſwelling betwixt the Ligature and the Neck of the Bladder, which had been purpoſely left open, as betwixt the ſame Ligature and the bottom of the Bladder, whence the included Air could no way get out.

But a further and ſufficient manifeſtation whence the intumeſſence of the Bladder proceeds, may be deduc'd from the following Experiment.

TO try then at once both what it was that expanded the Bladder, and what a powerful Spring there is ev'n in the Air *Experiment* 5.

we are wont to think uncompreſſ'd, we cauſ'd a Bladder dry, well ty'd and blown moderately full, to be hung in the Receiver by one end of a ſtring, whoſe other end was faſtned to the inſide of the Cover: and upon drawing out the ambient Air, that preſſ'd on the Bladder; the internal Air not finding the wonted reſiſtance, firſt ſwell'd and diſtended the Bladder, and then broke it, with ſo wide and crooked a rent, as if it had been forcibly torn aſſunder with hands. After which a ſecond Bladder being convey'd in, the Experiment was repeated with like ſucceſs: And I ſuppoſe it will not be imagin'd that in this caſe the Bladder was broken by its own Fibres, rather then by the Impriſon'd Air.

And of this Experiment theſe two *Phænomena* may be taken notice of: The one, that the Bladder at its breaking gave a great report, almoſt like a Craker: And the other, That the Air contain'd in the Bladder, had the power to break it with the mention'd Impetuoſity, long before the ambient Air was, all or near all, drawn out of the Receiver.

But, to verifie what we ſay in another Diſcourſe, where we ſhow, That even true

true Experiments may, by reaſon of the eaſie miſtake of ſome unheeded Circumſtance, be unſucceſsfully try'd; we will Advertiſe, on this occaſion, that we did oftentimes in vain try the breaking of Bladders, after the manner above-mention'd: Of which the cauſe appear'd to be this, That the Bladders we could not break, having been brought us ready blown from thoſe that ſold them, were grown dry before they came to our hands: whence it came to paſs, that, if we afterwards ty'd them very hard, they were apt to fret and ſo become unſerviceable; and if we ty'd them but moderately hard, their ſtiffneſs kept them from being cloſ'd ſo exactly, but that when the included Air had in the exhauſted Receiver diſtended them as much as eaſily it could, it would in part get out between the little wrinkles of the Sphincter of the Neck: Whence alſo it uſually happen'd, that, upon the letting in the Air from without, the Bladders appear'd more flaccid and empty then before they were put in; whereas when the Bladders were brought us moiſt from the Butchers, we could, without injuring them, tye their necks ſo cloſe, that none of the Air once

blown in, could get out of them, but by violently breaking them.

It will not be amiſs on this occaſion to point at ſomething which may deſerve a more deliberate Speculation then we can now afford it; namely that the Elaſtical Power of the ſame Quantity of Air may be as well Encreaſ'd by the Agitation of the Aërial Particles (whether onely moving them more ſwiftly and ſcattering them, or alſo extending or ſtretching them out, I determine not) within an every way incloſing and yet yielding Body; as Diſplay'd by the withdrawing of the Air that preſſ'd it without. For we found that a Bladder, but moderately fill'd with Air and ſtrongly ty'd, being a while held near the Fire, not onely grew exceeding turgid and hard, but afterwards, being approach'd nearer to the Fire, ſuddenly broke with ſo loud and vehement a noiſe, as ſtony'd thoſe that were by, and made us, for a while after, almoſt deaf.

Experiment 6. HAving thus ſeen that the Air has an Elaſtical Power, we were next deſirous to know in ſome meaſure how far a

parcel

parcel of Air might by this its own Spring be dilated. And though we were not provided of Instruments fit to measure the dilatation of the Air any thing accurately, yet because an imperfect measure of it was more desireable then none at all, we devis'd the following Method as very easily practicable.

We took a limber Lambs Bladder which was thorowly wetted in fair Water, that the sides of it being squeez'd together, there might be no Air left in its folds: (as indeed we could not afterwards upon tryal discern any) The neck of this Bladder was strongly tyed about that of a small Glass, (capable of holding five full drachmes of Water) the Bladder being first so compress'd, that all the included Air was onely in the Glass, without being press'd there; then the Pump being set awork after a few exsuctions, the Air in the little Viol began to dilate it self and produce a small Tumor in the Neck of the Bladder; and as the ambient Air was more and more drawn away, so the included Air penetrated further and further into the Bladder, and by degrees lifted up the sides and display'd its folds, till at length it seem'd to have blown it

up to its full extent: whereupon the external Air, being permitted to flow back into the Reciver, repulſ'd the Air that had fill'd the Bladder into its former narrow receptacle, and brought the Bladder to be again flaccid and wrinkled as before: Then taking out the Bladder, but without ſevering it from the Glaſs, we did by a hole made at the top of the Bladder fill the Veſſel they both made up with Water, whoſe weight was five Ounces five Drachmes and an half: Five Drachmes whereof were above-mention'd to be the contents of the Bottle. So that in this Experiment, when the Air had moſt extended the Bladder, it poſſeſſ'd in all above nine times as much room as it did when it was put into the Receiver. And it would probably have much inlarg'd its bounds, but that the Bladder by its weight and the ſticking together of its ſides did ſomewhat reſiſt its expanſion: And which was more conſiderable, the Bladder appear'd tumid enough, whilſt yet a pretty deal of Air was left in the Receiver, whoſe exſuction would, according to our former Obſervation, probably have given way to a further expanſion of the Air, eſpeci-

ally

ally ſuppoſing the dilatation not to be reſtrain'd by the Bladder.

SInce we wrote the other day the former Experiment, we have met with ſome Glaſſes not very unfit for our purpoſe; by means of which we are now able, with a little more trouble, to meaſure the expanſion of the Air a great deal more accurately then we could by the help of the above-mention'd Bladder, which was much to narrow to allow the Air its utmoſt diſtention.

We took then firſt a Cylindrical Pipe of Glaſs, whoſe bore was about a quarter of an Inch in Diameter: this Pipe was ſo bent and doubled, that, notwithſtanding its being about two foot in length, it might have been ſhut up into a ſmall Receiver, not a Foot high: But by miſfortune it crack'd in the cooling, whereby we were reduced to make uſe of one part which was ſtraight and intire, but exceeded not ſix or ſeven Inches. This little Tube was open at one end; and at the other, where it was Hermetically ſeal'd, had a ſmall Glaſs bubble to receive the Air whoſe dilatation was to be meaſur'd.

Along the ſide of this Tube was paſted a ſtraight narrow piece of Parchment, divided into twenty ſix equal parts, marked with black Lines and Figures, that by them might be meaſur'd both the included Air and its dilatation. Afterwards we fill'd the Tube with Water almoſt to the top, and ſtopping the open end with a Finger, and inverting the Tube, the Air was permitted to aſcend to the abovemention'd Glaſs bubble. And by reaſon this aſcent was very ſlow, it gave us the opportunity to mark how much more or leſs then one of the twenty ſix diviſions this Air took up. By this means, after a tryal or two, we were inabled to convey to the top of the Glaſs a bubble of Air equal enough, as to ſight, to one of thoſe Diviſions: Then the open end of the Tube being put into a ſmall Viol, whoſe bottom was cover'd with Water about half an Inch high; we included both Glaſſes into a ſmall and ſlender Receiver, and cauſed the Pump to be ſet awork. The event was, That at the firſt exſuction of the Air there appear'd not any expanſion of the bubble, comparable to what appear'd at the ſecond, and that upon a very few exſuctions the bubble

reaching

reaching as low as the surface of the subjacent Water, gave us cause to think that if our Pipe had not been broken it would have expanded it self much further: Wherefore we took out the little Tube, and found that besides the twenty six divisions formerly mention'd, the Glass bubble and some part of the Pipe to which the divided Parchment did not reach, amounted to six divisions more. Whereby it appears that the air had taken up one and thirty times as much room as before, and yet seem'd capable of a much greater expansion, if the Glass would have permitted it. Wherefore, after the former manner, we let in another bubble, that by our guess was but half as big as the former, and found, that upon the exsuction of the Air from the Receiver, this little bubble did not onely fill up the whole Tube, but (in part) break through the subjacent Water in the Viol, and thereby manifest it self to have possessed sixty and odde times its former room.

These two Experiments are mention'd to make way for the more easie belief of that which is now to follow. Finding then that our Tube was too short to serve our turn, we took a slender Quill of Glass

which

which happen'd to be at hand, though it were not so fit for our purpose as we could have wished, in regard it was three or four times as big at one end as the other. This Pipe which was thirty Inches long, being Hermetically seal'd at the slender end, was almost filled with Water; and after the above-related manner a bubble was convey'd to the top of it, and the open extream was put into a Viol that had a little fair Water at the bottom: Then the Cover, by means of a small hole purposely made in it for the Glass Pipe to stand out at, was cemented on to the Receiver, and the Pump being set awork, after some exsuctions, not onely the Air manifestly appear'd extended below the surface of the subjacent Water; but one of the By-standers affirms, that he saw some bubbles come out at the bottom of the Pipe and break through the Water. This done, we left off Pumping, and observ'd how at the unperceiv'd leaks of the Receiver the Air got in so fast, that it very quickly impell'd up the Water to the top of the Tube, excepting a little space whereinto that bubble was repuls'd, which had so lately possess'd the whole Tube; this Air at the slender end appear'd

pear'd to be a Cylinder of $\frac{5}{6}$ parts of an Inch in length; but when the Pipe was taken out and turn'd upside down, it appear'd at the other end inferior in bulk to a Pea.

These things being thus done we took (to make the Experiment the more exactly) a small pair of Scales, such as Gold-Smiths use to weigh Gold Coyn in; and weighing the Tube and Water in it, we found them to amount to one Ounce thirty Grains and an half: Then we pour'd in as much Water as serv'd to fill up the Tube, wherein before we had left as much space unfill'd up as was possess'd by the bubble; and weighing again the Pipe and Water, we found the weight increas'd onely by one Grain. Lastly, pouring out the Water, and carefully freeing the Pipe from it (which yet we could not perfectly doe) we weighed the Glass alone, and found it to want two Drachmes and thirty two Grains of its former weight: So that the bubble of Air taking up the room but of one Grain in weight of Water, it appear'd that the Air by its own ἐλατὴρ was so rarified, as to take up one hundred fifty two times as much room as it did before: though it were then compress'd by

nothing but the ordinary pressure of the contiguous Air. I know not whether it be requisite to take notice, that this Experiment was made indeed in a moist Night, but in a Room, in whose Chimney there was burning a good Fire, which did perhaps somewhat rarifie the Air of which the bubble consisted.

It has seem'd almost incredible which is related by the Industrious *Mersennus*, That the Air by the violence of heat, though as great as our Vessels can support without fusion, can be so dilated as to take up seventy times as much room as before: Wherefore because we were willing to have a confirmation of so strange a *Phænomenon*; we once more convey'd into the Tube a bubble of the bigness of the former, and prosecuting the Experiment as before with the same Water, we observed that the Air did manifestly stretch it self so far, as to appear several times a good way below the surface of the Water in the Viol, and that too with a surface very convex toward the bottom of the Pipe. Nay, the Pump being ply'd a little longer, the Air did manifestly reach to that place where the bottom of the Tube lean'd upon the bottom of the Viol, and

ſeem'd to knock upon it and rebound from it: Which Circumſtances we adde, partly that the *Phænomenon* we have been relating may not be imputed to the bare ſubſiding of the Water that fill'd the Tube, upon the taking off the preſſure of the ambient Air. And partly alſo that it may appear that if our Experiments have not been as accurately made as with fitter Inſtruments might perhaps be poſſible; yet the expanſion of the Air is likely to be rather greater then leſſer then we have made it: Since the Air was able to preſs away the Water at the bottom of the Pipe, though that were about two Inches below the ſurface of the Water that was then in the Viol, and would have been at leaſt as high in the Pipe, if the Water had onely ſubſided and not been depreſſed: So that it ſeems not unlikely that if the Experiment could be ſo made, as that the expanſion of the Air might not be reſiſted by the Neighboring Bodies, it would yet inlarge its bounds, and perhaps ſtretch it ſelf to two hundred times its former bulk, if not more. However, what we have now try'd will, I hope, ſuffice to hinder divers of the *Phænomena* of our Engine from being diſtruſted:

Since in that part of the Atmoſphere we live in, that which we call the free Air (and preſume to be ſo uncompreſſ'd) is crouded into ſo very ſmall a part of that ſpace, which if it were not hindred it would poſſeſs. We would gladly have tryed alſo whether the Air at its greateſt expanſion could be further rarified by heat; but do what we could, our Receiver leak'd too faſt to let us give our ſelves any ſatisfaction in that particular.

Experiment 7.

TO diſcover likewiſe by the means of that preſſure of the Air, both the ſtrength of Glaſs, and how much intereſt the Figure of a Body may have in its greater or leſſer Reſiſtance to the preſſure of other Bodys, we made theſe further tryals.

We cauſ'd to be blown with a Lamp a round Glaſs bubble, capable of containing, by gueſs, about five Ounces of Water, with a ſlender neck about the bigneſs of a Swans Quill, and it was purpoſely blown very thin, as Viols made with Lamps are wont to be, that the thinneſs of the matter might keep the roundneſs of the Figure from making the Veſſel too

ſtrong. Then having moderately emptyed the Receiver, and taken it out of the Pump, we ſpeedily applyed to the Orifice of the bottom of it the Neck of the newly mention'd Glaſs, carefully ſtopping the Crannys with melted Plaiſter, that no Air might get in at them: And after turning the Key of the Stop-cock, we made a free paſſage for the Air to paſs out of the bubble into the Receiver: which it did with great celerity, leaving the bubble as empty as the Receiver it ſelf; as appear'd to us by ſome Circumſtances not now to be inſiſted on. Notwithſtanding all which, the Veſſel, continuing as intire as before, gave us cauſe to wonder that the bare Roundneſs of the Figure ſhould inable a Glaſs, almoſt as thin as Paper, to reſiſt ſo great a preſſure as that of the whole incumbent Atmoſphere. And having reiterated the Experiment, we found again that the preſſure of the ambient Body, thruſting all the parts inwards, made them, by reaſon of their arched Figure, ſo ſupport one another, that the Glaſs remain'd as whole as at firſt.

Now that the Figure of the Glaſs is of great moment in this matter, may be evinced by this other Experiment.

Experiment 8.

VVE took a Glaſs Helmet or Alembick (delineated by the ſeventh Figure) ſuch as Chymiſts uſe in Diſtillations, and containing by conjecture between two and three Pints: The *Roſtrum* or Noſe of it mark'd with (*c*) was Hermetically cloſed; and at the top of it was a hole, into which was fitted and cemented one of the Shanks of a middle-ſiz'd Stop-cock; ſo that the Glaſs being turn'd upſide-down, the wide Orifice (which in common Glaſs-Helmets is the onely one) was upwards; and to that wide Orifice was fitted a caſt-Cover of Lead, which was carefully cemented on to the Glaſs: Then the other Shank of the Stop-cock being with Cement likewiſe faſten'd into the upper part of the Pump, the exſuction of the Air was endeavoured. But it was not long before, the remaining Air being made much too weak to ballance the preſſure of the ambient Air, the Glaſs was not without a great noiſe crack'd almoſt half round, along that part of it where it began to bend inwards: As if in the Figure the crack had been made according to the Line (*a b*); and upon an

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endeavour to pump out more of the Air, the crack once began, appear'd to run on further; though the Glaſs where it was broken ſeem'd to be by conjecture above ten, ſome thought above twenty times as thick as the bubble mention'd in the foregoing Experiment.

This will perhaps make it ſeem ſtrange, that having taken another Glaſs bubble blown at the ſame time, and like for ought we diſcern'd for ſize, thickneſs and Figure to that thin one formerly mention'd; and having ſeal'd it up Hermetically, and ſuſpended it in the Receiver, the exſuction of the ambient Air did not enable the impriſoned Air to break, or in the leaſt to crack the bubble; though the Experiment were laboriouſly try'd, and that ſeveral times with bubbles of other ſizes: But that perhaps the heat of the Candle or Lamp wherewith ſuch Glaſſes are Hermetically ſeal'd, (not to mention the warmth of his hands that ſeal'd it) might ſo rarifie the contained Air, as much to weaken its Spring, may ſeem probable by the following Experiments.

Experiment 9. WE took a Glaſs Viol able to hold three or four Ounces of Water, and of the thickneſs uſual in Glaſſes of that ſize; into the Neck of this was put a moderately ſlender Pipe of Glaſs, which was carefully faſten'd with a mixture of equal parts of Pitch and Roſin to the Neck of the Viol, and which reach'd almoſt to the bottom of it, as the ſixth Figure declares.

This Viol being upon a particular deſign fill'd with Water, till that came up in it a pretty deal higher then the lower end of the Pipe, was put into one of our ſmall Receivers, (containing between a Pint and a Quart) in ſuch manner as that the Glaſs Pipe, paſſing through a hole made purpoſely for it in the Leaden-Cover of the Receiver, was for the moſt part of it without the Veſſel, which being exactly cloſed, the Pump was ſet awork: But at the very firſt exſuction, and before the Sucker was drawn to the bottom of the Cylinder, there flew out of the Viol a piece of Glaſs half as broad as the Palm of a Mans Hand, and it was thrown out with ſuch violence, that hitting againſt

the

the Neighboring ſide of the Receiver, it not onely daſh'd it ſelf to pieces, but crack'd the very Receiver in many places, with a great noiſe that much ſurpriſed all that were in the Room. But it ſeem'd that in ſo little a Receiver, the Air about the Viol being ſuddenly drawn out, the Air Impriſon'd in the Veſſel, having on it the whole preſſure of the Atmoſphere (to which by the Pipe open at both ends, It and the Water were expoſ'd) and not having on the other ſide the wonted preſſure of the Ambient Air to ballance that other preſſure, the reſiſtance of the Glaſs was finally ſurmounted, and the Viol once beginning to break where it was weakeſt, the external Air might ruſh in with violence enough to throw the crack'd parcel ſo forcibly againſt the Neighboring ſide of the Receiver, as to break that too.

And this may be preſumed ſufficient to verifie what we delivered in that part of our Appendix to the firſt Experiment, where we mention'd the almoſt equal preſſure of the Air on either ſide of a thin Glaſs Veſſel, as the cauſe of its not being broken by the forcible Spring of the contain'd Air. But yet that it be not ſuſpected that chance had an intereſt in ſo

odde an Experiment as we have been Relating, we will adde that for farther ſatiſfaction we reiterated it in a round Glaſſe containing by gueſſe about ſix ounces of water: this violl we put into ſuch a ſmall Receiver as was lately mention'd, in ſuch manner as that the bottome of it reſted upon the lower part of the Pneumaticall Glaſſe, and the Neck came out through the Leaden-Cover of the ſame at a hole made purpoſely for it. But being made circumſpect by the foregoing miſchance, we had put the violl into a Bladder, before we put it into the Receiver to hinder this laſt named Glaſſe from being endanger'd by the breaking of the other. Then the Pneumaticall veſſell being clos'd ſo that no way was left for the outward Air to get into it, but by breaking through the Viol, into whoſe cavity it had free acceſſe by the mouth of it, (which was purpoſely left open,) the Sucker being nimbly drawn down, the external Air immediatly preſſ'd forcibly as well upon the Leaden-Cover as the Violl; and the Cover happening to be in one place a little narrower then the edge of the Pneumatical Glaſs, was depreſſ'd, and thruſt into it ſo vio-

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lently by the incumbent Air, that getting a little within the tapering Lip of the Glaſs, it did like a kinde of Wedge, thruſt out that ſide where it was depreſſ'd, ſo as, though the Receiver was new, to ſplit it. This accident being thus mention'd upon the by to confirm what we formerly ſaid touching the fitneſs or unfitneſs of Glaſſes of ſome Figures to reſiſt the preſſure of the Atmoſphere; We will proceed to relate the remaining part of the Experiment, namely, That having fitted on a wider Cover to the ſame Receiver, and cloſed both that and the crack with Cement, we proſecuted the Experiment in the manner above related, with this ſucceſs: That upon the quick depreſſing of the Sucker, the external Air burſt the Body of the Viol in above a hundred pieces, many of them exceeding ſmall, and that with ſuch violence that we found a wide rent, beſides many holes, made in the Bladder it ſelf.

And to evince that theſe *Phænomena* were the effects of a limited and even moderate force, and not of ſuch an abhorrency of a *Vacuum* as that to avoid it, many have been pleaſed to think that Nature muſt, upon occaſion, exerciſe an al-

most boundless power; we afterwards purposely try'd this Experiment with several Glasses somewhat thicker then those Viols, and found the event to verifie our conjecture, that it would not succeed: for the Glasses were taken out as intire as they were put in.

And here, My Lord, I hold it not unfit, upon occasion of the mention that has been made of our having employ'd small Receivers, and one of them, notwithstanding its being crack'd, to annex these two Advertisements.

First then, besides the great Pneumatical Glass so often mention'd, and the proportionate Stop-cock, we thought fit to provide our selves of some small Receivers blown of Crystalline Glass, of severall Shapes, and furnished with smaller Stop-cocks purposely made; and this we did upon hopes that when we had surmounted the difficulties to be met with in Cementing the Glasses to the Stop-cocks, and the Pneumatical Vessels to the Pump so exquisitely as is requisite for our purpose, we should from the smalness of our Receivers receive a four-fold Advantage. The first, that by reason of the slenderness of the Vessels, and their being

ing made of much purer and clearer metall, as the Glaſs-men ſpeak, then the great Receiver, we might have a more perfect view of every thing happening within them. The next, that ſuch ſmall Veſſels might be empty'd with leſs labour and in much leſſe time. The third, that this nimble exſuction of the ambient Air would make many changes in the Bodies ſhut up in theſe glaſſes more ſudden and conſpicuous then otherwiſe they would prove. And the laſt, that we ſhould be able to draw and keep out the Air much more perfectly from ſuch ſmall Veſſels then from our large Receiver. But though we were not much diſ-appointed in the expectation of the three firſt advantages, yet we were in our hopes of the fourth. For beſides the great difficulty we found in fitting together the Glaſſes, the Stop-cocks and the Covers; beſides this I ſay, we found our ſelves ſeldom able to draw, and keep out the Air ſo far as to make the remaining Air in theſe Receivers weaker then the remaining Air in our great Receiver. For though ſometimes the Leaks of ſome of theſe little Receivers may be much either fewer or ſmaller then thoſe of the larger Veſſel; yet a little Air get-

ting into one of theſe, wherein it had but little room to expand and diſplay it ſelf, might preſs as much upon all parts of the internal ſurface of the Veſſel, and upon the included Bodies, as a greater quantity of Air in a Veſſel in whoſe capacity it might finde more room to expand it ſelf.

The other thing that we were to advertiſe, is, That 'tis not every ſmall crack that can make ſuch a Receiver as is of a roundiſh Figure altogether uſeleſs to our Experiment, in regard that upon the exſuction of the internal Air, the ambient Air on all ſides preſſing the Glaſs inwards or towards the middle, does conſequently thruſt the Lips of the crack cloſer, and ſo rather cloſe then increaſe it.

This I mention partly becauſe Receivers fit for our turn are more eaſily crack'd then procur'd, and therefore ought not to be unneceſſarily thrown away as unſerviceable: And partly becauſe I think it becomes one that profeſſes himſelf a faithful Relator of Experiments, not to conceal from Your Lordſhip, that after a few of the foregoing Experiments were made, there happen'd in the great Receiver a crack of about a Span long, begin-

ning

ning at the upper Orifice, and occasion'd, as it seem'd, by the excessive heat of too large an Iron that was employ'd to melt the Cement about that Orifice. But having laid upon this crack a broad Plaister, which in one of our Essays written some years since to your ingenious and hopeful Cousin *Jones*, we extoll for the mending of crack'd Receivers, and other Chymical Glasses; and having afterwards thickly over-laid this Plaister with Diachylon, we neither could then, nor can yet perceive that the Vessel leaks sensibly at that crack.

The Plaister was made of good quick Lime finely poudred, and nimbly ground with a Pestle in a Morter, with a quantity (I know not how much precisely, not having those Essays in this place) of scrapings of Cheese and a little fair Water, no more then is just necessary to bring the mixture to a somewhat soft Paste, which when the Ingredients are exquisitely incorporated, will have a strong and stincking smell: Then it must be immediately spread upon a Linnen Cloath three or four fingers breadth, and presently apply'd, lest it begin to harden. But if Your Lordship had seen how we mended

with

with it Receivers even for the most subtle Chymical Spirits, You would scarce wonder at the service it has done in our Pneumatical Glass.

Experiment 10.

WE took a Tallow-Candle of such a size that eight of them make about a pound, and having in a very commodious Candlestick let it down into the Receiver, and so suspended it that the Flame burnt almost in the middle of the Vessel, we did in some two minutes exactly close it up: and, upon Pumping very nimbly, we found that within little more then half a minute after the Flame went out, though the Snuff had been purposely left of that length we judged the most convenient for the lasting of the Flame.

But the second time having put in the same Candle into the Receiver, (after it had by the blasts of a pair of Bellows been freed from Fumes) the Flame lasted about two minutes from the time the Pumper began to draw out the Air; upon the first exsuction whereof, the Flame seem'd to contract it self in all its Dimensions. And these things were further ob-

servable,

ſervable, that after the two or three firſt exſuctions of the Air, the Flame (except at the very top) appear'd exceeding blew, and that the Flame ſtill receded more and more from the Tallow, till at length it appear'd to poſſeſs onely the very top of the Week, and there it went out.

The ſame Candle being lighted again was ſhut into the Receiver, to try how it would laſt there without drawing forth the Air, and we found that it laſted much longer then formerly; and before it went out receded from the Tallow towards the the top of the Week, but not near ſo much as in the former Experiment.

And having an intention to obſerve particularly what the motion of the ſmoak would be in theſe Experiments: We took notice that when the Air was not drawn out, there did upon the extinction of the Flame a conſiderable part of the Week remain kindled, which (probably by reaſon of the Circulation of the Air in the Veſſel, occaſion'd by the heat) emitted a Steam, which aſcended ſwiftly and directly upwards in a ſlender and uninterrupted Cylinder of ſmoke, till it came to the top, whence it manifeſtly recoyl'd by the ſides to the lower part of the Veſ-

fel. Whereas when the Flame went out upon the exſuction of the Air one time (when the Flame retir'd very leaſurely to the top) we perceived it not to be follow'd by any ſmoke at all. And at another time the upper part of the Week remaining kindled after the extinction of the Flame, the ſlender ſteam of Fumes that did ariſe aſcended but a very little way, and then after ſome uncertain motions this and that way, did, for the moſt part, ſoon fall downwards.

Being deſirous alſo to try whether there would be any difference as well in our Receiver as there is wont to be elſewhere betwixt Candles made of Wax and thoſe made of Tallow, as to their duration; we took ſlender Tapers of white Wax, (commonly called Virgins Wax) that being found to burn with much leſs ſmoke then common yellow Wax: Six of theſe of like bigneſs, and each of them of about the thickneſs of a Swans Quill, we preſſ'd together into one Candle: And having lighted all the Weeks, we let in the above-mention'd Wax into the Receiver, and made what haſte we could to cloſe it up with Cement. But though in the mean while we left open the Valve

of

of the Cylinder, the hole of the Stop-cock and that in the Cover of the Receiver, that ſome Air might get in to cheriſh the Flame and the ſmoke might have a vent; Yet for ſo great a Flame the Air ſufficed not ſo much as till the Cover could be perfectly luted on: So that before we were quite ready to imploy the Pump, the Candle was extinguiſhed. Wherefore we took but one of the above mention'd Tapers, and having lighted it, cloſ'd it up in the Receiver, to try how long a ſmall Flame with a proportionable ſmoke would continue in ſuch a quantity of Air: But we found upon two ſeveral tryals, that from the beginning of pumping, the Flame went out in about a minute of an hour. It appear'd indeed to us that the ſwinging of the Wier to and fro (in the Engine ſhaken by pumping) haſten'd the vaniſhing of the Flame, which ſeem'd by that motion to be caſt ſometimes on one ſide of the Week and ſometimes on the other: But though once we purpoſely refrain'd pumping after a very few exſuctions of the Air, that the Flame might not be agitated, yet it laſted not much longer then the newly mention'd time.

And

And laſtly, cloſing up the ſame Taper, lighted again, to diſcover how long it would laſt without drawing out of the Air, we found that it burn'd for a while vividly enough, but afterwards began to be leſſen'd more and more in all its Dimenſions. And we obſerv'd that the Flame did not, as before, retire it ſelf by little and little towards the top, but towards the bottom of the Week (from which yet it did a little withdraw upwards juſt before it went out) ſo that the upper part of the Week appear'd for a pretty while manifeſtly above the top of the Flame, which having laſted about five minutes, was ſucceeded by a directly aſcending ſtream of Smoak.

Experiment 11. THere was taken a Wier, which being bent almoſt in the form of a Screw, conſtituted ſuch an Inſtrument to contein Coals and leave them every way acceſſible to the Air, as the tenth Figure declares; the breadth of this Veſſel was no leſs then that it might with eaſe be convey'd into the Receiver: And having filld it to the height of about five Inches with throughly kindled Wood-coals, we let

it down into the Glaſs; and ſpeedily cloſing it, we cauſ'd the Pumper to ply his work, and obſerv'd that upon the very firſt exſuction of the Air (though perhaps not becauſe of that onely) the Fire in the Coals began to grow very dim, and though the agitation of the Veſſel did make them ſwing up and down (which in the free Air would have retarded the extinction of the Fire) yet when we could no longer diſcern any redneſs at all in any of them, caſting our eyes upon a Minute-Watch we kept by us on this occaſion, we found that from the beginning of the Pumping (which might be about two minutes after the Coals had been put in glowing) to the total diſ-appearing of the Fire, there had paſſed but three minutes.

Whereupon, to try the Experiment a little further, we preſently took out the Coals, in which it ſeems there had remained ſome little parcels of Fire, rather cover'd then totally quench'd: For in the open Air the Coals began to be re-kindled in ſeveral places, wherefore having by ſwinging them about in the Wier, throughly lighted them the ſecond time, we let them down again into the Receiver,

and clos'd it speedily as before; and then waiting till the Fire seem'd totally extinct without medling with the Pump, we found that from the time the Vessel was clos'd till that no Fire at all could be perceiv'd there had passed about four minutes: Whereby it seem'd to appear that the drawing away of the ambient Air made the Fire go out sooner then otherwise it would have done; though that part of the Air that we drew out left the more room for the stifling steams of the Coals to be received into.

Lastly, Having taken out the Wier and put other Coals into it, we did, in the same Room where the Engine stood, let it hang quietly by a string in the open Air, to try how long the Fire would last without agitation when no Air was kept from it. And we found that the Fire began to go out first at the top and out-sides of the Coals; but inwards and near the bottom the Fire continu'd visible for above half an hour, a great part of the Coals, especially those next the bottom, being burnt to ashes before the Fire went out.

We caus'd likewise a piece of Iron to be forg'd, of the bigness of a middle siz'd Char-coal, and having made it red hot

throughout; we caus'd it in the lately mention'd Wier, to be speedily convey'd and shut up into the Receiver, being desirous to try what would become of a glowing Body, by reason of its texture more vehemently hot then a burning Coal of the same bigness, & yet unlike to send forth such copious & stifling Fumes: But we could not observe any manifest change upon the exsuction of the Air. The Iron began indeed to lose its Fiery redness at the top, but that seem'd to be because it was at the upper end somewhat more slender then at the lower: The redness, though it were in the day time, continued visible about four minutes; and then, before it did quite dis-appear, we turn'd the Key of the Stop-cock but could not discern any change of the Iron upon the rushing in of the Air. Yet some little remainders of Wax that stuck to the Wier, and were turn'd into Fumes by the heat of the neighboring Iron, seem'd to afford a more plentiful, or at least a much more free expanded smoke when the Air was suck'd out, then afterwards; though allowance was made for the decreasing heat of the Iron. And lastly, notwithstanding a considerable exsuction

of the ambient Air, though not by far ſo great a one as might have been made by the Engine; and notwithſtanding the inconſiderable diſſipation of the parts of the Iron, the ſurrounding ſides of the Receiver were ſenſibly, and almoſt offenſively heated by it; inſomuch that a pretty while after the Iron was taken out, the ſides of the Glaſs manifeſtly retain'd a warmth: which would not be unfit to be conſider'd by a Perſon at more leaſure then I am now.

Experiment 12.

BEing willing to try after this ſomething that would not cheriſh much Fire at once, and would keep Fire much longer then a Coal. We took a piece of Match, ſuch as Souldiers uſe, of the thickneſs of a Mans little Finger, or ſomewhat thicker; and this being well lighted at one end, was by a ſtring ſuſpended with that end downwards in the cavity of the Receiver which was immediately cloſ'd: And yet by that time it could well be ſo, the copious Fumes of the Match had neer fill'd and darken'd the Receiver. Wherefore, leſt the Veſſel ſhould be endanger'd, the Pump was nimbly ply'd, and a great

deal of Air and Smoke mixt together was drawn out, whereby the Receiver growing more clear, we could diſcern the Fire in the Match to burn more and more languidly: And notwithſtanding that by the diligence uſ'd in Pumping, it ſeem'd to have room enough allow'd it to throw out Fumes; yet after no long time it ceaſ'd from being diſcernable either by its Light or its Smoke. And though by that we were invited to ſuppoſe it quite extinguiſhed, yet we continu'd pumping a while, in proſecution of another Experiment we were trying at the ſame time: And this we did the more willingly becauſe of a ſuſpicion the Experiment about the Coals might eaſily ſuggeſt, and which the event declar'd not to have been altogether groundleſs. For upon the Admiſſion of the external Air, the Fire, that ſeem'd to have gone out a pretty while before, did preſently revive; and being as it were refreſh'd by the new Air, and blown by the Wind made by that Air in ruſhing in, it began again to ſhine and diſſipate the neighboring Fuel into Smoke as formerly.

Experiment 13.

A While after we let down into the Receiver together with a lighted piece of Match, a great Bladder well tyed at the Neck, but very lank, as not containing actually much (if any thing) above a Pint of Air, but being capable of containing ten or twelve times as much.

Our ſcope in this Experiment was partly to try whether or no the ſmoke of the Match, repleniſhing the Receiver, would be able to hinder the Dilatation of the inward Air, upon the exſuction of the Ambient. And partly to diſcover whether the extinction of the Fire in the Match did proceed from want of Air, or barely from the preſſure of its own Fumes, which for want of room to expand themſelves in, might be ſuppoſ'd to Recoyl upon the Fire, and ſo to ſtifle it.

The event of our tryal was, That at the beginning of our Pumping the Match appear'd well lighted, though it had almoſt fill'd the Receiver with its plentiful Fumes: But by degrees it burnt more and more dimly, notwithſtanding that by the nimble drawing

out the Air and Smoke, the Veſſel were made leſs opacous, and leſs full of compreſſing matter; as appeard by this; That the longer we pump'd, the leſſer Air and Smoke came out of the Cylinder at the opening the Valve, and conſequently the leſs came into it before; yet the Fire in the Match went but ſlowly out. And when afterwards, to ſatisfie our ſelves of its expiration, we had darken'd the Room, and in vain endeavored to diſcover any ſpark of Fire, as we could not for ſome time before by the help of Candles diſcern the leaſt riſing of Smoke, we yet continued pumping ſix or ſeven times; and after all that letting in the Air, the ſeemingly dead Fire quickly revived, and manifeſted its recovery by Light and ſtore of Smoke, with the latter of which it quickly began to replenish the Receiver. Then we fell to pumping afreſh, and continued that labour ſo long till the re-kindled Match went out again: and thinking it then fit not to ceaſe from Pumping ſo ſoon as before, we found that in leſs then half a quarter of an hour the Fire was got out for good and all,

and

and past the possibility of being recover'd by the re-admitted Air.

Some Circumstances, besides those already mention'd, occurr'd in the making of the Experiment, of which these are the principal.

First, when the Receiver was full of Smoke, if the Cylinder were emptied, immediately upon the turning of the Stop-cock, the Receiver would appear manifestly darken'd to his eye that look'd upon the light through it: and this darkness was much less when the Receiver was much less fill'd with Fumes: It was also instantaneous, and seem'd to proceed from a sudden change of place and scituation in the exhalations, upon the vent suddenly afforded them and the Air they were mixt with, out of the Receiver into the Cylinder.

The next thing we observ'd was, a kinde of *Halo* that appear'd a good while about the Fire, and seem'd to be produced by the surrounding Exhalations.

And lastly, it is remarkable, That even when the Fumes seemed most to replenish the Receiver, they did not sensibly hinder the Air included in the Bladder from dilating it self after the same manner

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(for ought we could difcern) as it would have otherwife done: So that before the Fire or the Match was quite extinct, the Bladder appear'd fwell'd at leaft to fix or feven times its former capacity.

Since the writing of thefe laft Lines, we took a fmall Receiver, capable of containing (by guefs) about a pound and a half of Water; and in the midft of it we fufpended a lighted Match, but though within one minute of an hour (or there-abouts) from the putting in of the Match, we had cemented on the Cover, yet we could not make fuch hafte, but that before we began to pump, the Smoke had fo fill'd that fmall Receiver, as for ought we difcern'd, to choke the Fire. And having again and again reiterated the Experiment, it feem'd ftill as at firft, that we could not clofe up the Veffel and pump out all the Fumes time enough to refcue the Fire from Extinction; whereupon we made ufe of this Expedient. Affoon as we had pump'd once or twice, we fudden-ly turn'd the Key, and thereby gave accefs to the excluded Air, which rufhing vio-lently in, as if it had been forced thorow a pair of Bellows, did both drive away the afhes, fill the Glafs with frefh Air, and

by blowing the almoſt extinguiſh'd Fire, re-kindl'd it, as appear'd by the Matches beginning again to ſmoke, which before it had ceaſ'd to do; we having by this means obtain'd a lighted Match in the Receiver, without being reduc'd to ſpend time to cloſe it up, commanded the Air to be immediatly pump'd out, and found that upon the exſuction of it, the Match quickly left ſmokeing, as it ſeem'd, by reaſon of the abſence of the Air; and yet if ſome urgent occaſions had not hinder'd us, we would for greater ſecurity have try'd, whether or no the Match rekindled as formerly, would ſmoke much longer, in caſe of no exſuction of the ambient Air.

Experiment 14.

TO try diverſe things at once, and particularly whether Fire, though we found it would not long laſt, might not yet be produced in our evacuated Receiver: We took a Piſtol of about a Foot in length, and having firmly tyed it to a ſtick almoſt as long as the Cavity of the Receiver, we very carefully prim'd it with well dry'd Gunpowder, and then cocking it, we ty'd to

the Tricker one end of a ſtring, whoſe other end was faſten'd to the Key formerly mention'd to belong to the Cover of our Receiver. This done, we convey'd the Piſtol, together with the annexed Staff, into the Veſſel: which being cloſ'd up, and empty'd after the uſual manner, we began to turn the Key in the Cover; and thereby ſhortning the ſtring that reach'd from it to the Piſtol, we pull'd aſide the Tricker, and obſerv'd, that according to our expectation the force of the Spring of the Lock was not ſenſibly abated by the abſence of the Air. (from whoſe *impetus* yet ſome Modern Naturaliſts would derive the cauſe of the motion of Reſtitution in ſolid Bodies) For the Cock falling with its wonted violence upon the Steel, ſtruck out of it as many and as conſpicuous parts of Fire, as, for ought we could perceive, it would have done in the open Air. Repeating this Experiment divers times, we alſo obſerved whether or no there would appear any conſiderable Diverſity in the Motion of the ſhining Sparks in a place where the remaining Aire was ſo much rarified, but could not perceive

but

but that they moved ſome of them upwards, as well as ſome of them downwards, and ſome of them ſide-ways, as they are wont to do, when upon ſuch colliſions they fly out in the open Air.

We likewiſe cauſ'd a piece of Steel to be made of the form and bigneſs of the Flint, in whoſe place we put it, and then the Piſtol being cock'd and conveyed into the Receiver, the Trigger was pull'd after the Air was drawn out: And though the place were purpoſely ſomewhat darken'd, yet there appear'd not upon the ſtriking of the two Steels againſt each other the leaſt ſpark of Fire: Nor did we expect any (having before in vain attempted to ſtrike Fire this way in the open Air) though we thought fit to make the Experiment to undeceive thoſe who fancy in rarified Air, I know not what ſtrange diſpoſition, to take Fire upon a much ſlighter occaſion then this Experiment afforded. We have indeed found, that by the dextrous Colliſion of two harden'd pieces of Steel, ſtore of ſparks may be ſtruck out: But that was done with ſuch vehement percuſſion of the edges of the two Steels, as could not well be compaſſ'd in our Receiver.

But

But the chief thing we design'd to do with our Pistol, was, To observe whether Gun-powder would take Fire in our empty'd and closely stop'd Glass? Whether the expansion of the Flame would be considerably varied by the absence of so much of the ambient Air as was drawn out of the Receiver? and whether the Flame would diffuse it self upward, as it is wont, notwithstanding its not having about it the usual proportion of Air to force it up? And though most of our attempts to fire the Gun-powder in the Pan of the Pistol succeeded not, because we were fain to let it hang almost perpendicular in the Receiver; whereby the Powder was shaken down before the sparks could reach it: yet once the Experiment succeeded, and the kindled Powder seem'd to make a more expanded Flame then it would have done in the open Air, but mounted upwards according to its wont; whether by reason of that little portion of Air, which in spight of our pumping remained in the Receiver; or for any other cause, we have not now the leisure to consider. But we must not forget, that upon the extinction of the Flame the Receiver appear'd darken'd with

smoke,

ſmoke, which ſeem'd to move freely up and down, and upon the letting in the Air at the Stop-cock began to circulate much faſter then before. We wonld have made more obſervations concerning this Flame, but that of two or three attempts we afterwards made to repeat the kindling of Powder, not any one ſucceeded; and we have not the leaſure to dwell long upon one kinde of Tryals.

Experiment. 15.

TO theſe Experiments concerning Fire we added another, which, though it ſucceded not, may perhaps without impertinency be recorded: partly becauſe that (as we have in another Treatiſe amply declar'd) it is uſefull to recite what Experiments miſcarry as well as what ſucceed. And partly alſo becauſe it is very poſſible that what we endeavored in vaine, may be performed by Your Lordſhip, or ſome other *Virtuoſo* that ſhall have ſlancker Veſſells then we had, and more Sunny dayes then the preſent Winter allows us.

We convey'd then into one of our ſmall Receivers a piece of matter combuſtible, dry and black (experience declaring things

of that colour to be most easily kindled) & carefully closing the Vessel we brought it to a Window at which the Sun, not very farre from the Meridian, shone in very freely: then drawing out the Aire with speed united the Sun-beames with a burning Glass upon the combustible matter which began immediatly to send forth a Smoke that quickly darkned the Receiver, but notwithstanding all our care and diligence the externall Aire got in so fast that after diverse tryals we were fayne to leave off the Experiment in that Glasse and induc'd to make tryall of it in our great Receiver.

Haveing then after some difficulty lodg'd the combustible matter in the cavity of this Vessell in such manner as that it was almost contiguous to that side thereof that was next the Sun, we did endeavor with a pretty large burning Glass to kindle it, but found, as we fear'd, That by reason of the thickness of the Glass, (which was also of a less pure and less Diaphanous matter. then the other) the Sun-beams thrown in by the burning Glass, were in their passage so Dislocated and Scattered (not now to mention those many that being reflected,

I could

I could not pierce into the cavity of the Receiver) that we could not poſſibly unite enough of them to kindle the matter, nor ſo much as to make it ſenſibly ſmoke. Yet we hope that the ſeeing whether Bodies (other then Gun-powder) may be kindled, and what would happen to them when ſet on fire, in a place in great meaſure devoid of Air, may prove ſo Luciferous an Experiment, that when the Seaſon is more favorable we ſhall, God permitting, make further tryal of it, and acquaint Your Lordſhip with the Event, if it prove proſperous. In the mean time we ſhall paſs on to other Experiments, aſſoon as we have advertiſ'd Your Lordſhip that we have forborn to make ſuch Reflections upon the ſeveral Experiments we have ſet down concerning Fire, as the matter would have eaſily enough afforded, and Your Lordſhip may perhaps have expected. But I made the leſs ſcruple to forbear the annexing of Speculations to theſe Recitals, becauſe *Carneades* & *Eleutherius* have in ſome Dialogues concerning Heat and Flame, which were laſt year ſeen by ſome Friends, and may be, when you pleaſe, commanded by You, mention'd

tion'd divers of my Thoughts and Experiments concerning Fire.

WE designed to try whether or no divers Magnetical Experiments would exhibit any unusual *Phænomena*, being made in our Evacuated Receiver instead of the open Air: But for want of leisure and conveniency to prosecute such Tryals, we were induced to reserve the rest for an other time, and to content our selves with making that which follows. We convey'd into the Receiver a little Pedestal of Wood, in the midst of which was perpendicularly erected a slender Iron, upon whose sharp point an excited Needle of Steel purposely made, and of about five Inches long, was so placed that hanging in an *Æquilibrium* it could move freely towards either hand. Then the Air being after the usual manner pumped out, we apply'd a Load-stone moderately vigorous to the out-side of of the Glass, and found that it Attracted or Repell'd the ends of the Needle, according to the Laws Magnetical, without any remarkable difference from what the same Load-stone would have done had

Experiment 16.

none

none of the Air been drawn away from about the Needle, which when the Loadstone was removed, after some tremulous Vibrations to and fro, rested in a position wherein it look'd North and South.

Experiment 17. PRoceed we now to the mention of that Experiment, whereof the satisfactory tryal was the principal Fruit I promis'd my self from our Engine. It being then sufficiently known, that, in the Experiment *De Vacuo*, the Quicksilver in the Tube is wont to remain elevated, above the surface of that whereon it leans, about 27 digits: I considered, that, if the true and onely reason why the Quick-silver falls no lower, be, that at that Altitude, the Mercurial Cylinder in the Tube, is an *Æquilibrium* with the Cylinder of Air, suppos'd to reach from the adjacent Mercury to the top of the Atmosphere: If this Experiment could be try'd out of the Atmosphere, the Quick-silver in the Tube would fall down to a levell with that in the Vessel, since then there would be no pressure upon the Subjacent, to resist the weight of the

the Incumbent Mercury. Whence I inferr'd (as easily I might) that, if the Experiment could be try'd in our Engine, the Quick-silver would subside below 27 Digits, in proportion to the exsuction of Air, that should be made out of the Receiver. For, as when the Air is shut into the Receiver, it does (according to what hath above been taught) continue there as strongly compress'd, as it did whil'st all the incumbent Cylinder of the Atmosphere lean'd immediatly upon it; because the Glass, wherein it is pent up, hinders it to deliver it self, by an expansion of its parts, from the pressure wherewith it was shut up. So, if we could perfectly draw the Air out of the Receiver, it would conduce as well to our purpose, as if we were allow'd to try the Experiment beyond the Atmosphere.

Wherefore (after having surmounted some little difficulties which occurr'd at the beginning) the Experiment was made after this manner. We took a slender and very curiously blown Cylinder of Glass, of near three Foot in length, and whose bore had in Diameter a quarter of an Inch, wanting a hairs breadth: This Pipe being Hermetically seal'd at one end, was, at

the other, fill'd with Quick-silver, care being taken in the filling, that as few bubles as was possible should be left in the Mercury: Then the Tube being stopt with the Finger and inverted, was open'd, according to the manner of the Experiment, into a somewhat long and slender Cylindrical Box (instead of which we now are wont to use a Glass of the same form) half fill'd with Quick-silver: And so, the liquid metal being suffered to subside, and a piece of Paper being pasted on levell with its upper surface, the Box and Tube and all were by strings carefully let down into the Receiver, and then, by means of the hole formerly mention'd to be left in the Cover, the said Cover was slip't along as much of the Tube as reach'd above the top of the Receiver; And the Interval, left betwixt the sides of the Hole and those of the Tube, was very exquisitely fill'd up with melted (but not over hot) Diachylon; and the round chink, betwixt the Cover and the Receiver, was likewise very carefully clos'd up: Upon which closure there appear'd not any change in the height of the Mercurial Cylinder; no more, then if the interpos'd Glass Receiver did not hinder the immediate pressure

of the ambient Atmoſphere upon the incloſed Air; which hereby appears to bear up on the Mercury, rather by virtue of its ſpring, then of its weight: ſince its weight cannot be ſuppoſ'd to amount to above two or three Ounces, which is inconſiderable in compariſon of ſuch a Cylinder of Mercury as it would keep from ſubſiding.

All things being thus in a readineſs, the Sucker was drawn down; and, immediately upon the egreſs of a Cylinder of Air out of the Receiver; the Quick-ſilver in the Tube did, according to expectation, ſubſide: and notice being carefully taken (by a mark faſten'd to the outſide) of the place where it ſtopt, we cauſ'd him that manag'd the Pump to pump again, and mark'd how low the Quick-ſilver fell at the ſecond exſuction; but continuing this work, we were quickly hindred from accurately marking the Stages made by the Mercury in its deſcent, becauſe it ſoon ſunk below the top of the Receiver; ſo that we could thenceforward mark it no other ways then by the eye. And thus, continuing the labor of pumping for about a quarter of an hour, we found our ſelves unable to bring the Quick-ſilver in

the Tube totally to subside; because, when the Receiver was considerably empty'd of its Air, and consequently that little that remain'd grown unable to resist the Irruption of the external, that Air would (in spight of whatever we could do) press in at some little Avenue or other; and though much could not thereat get in, yet a little was sufficient to counterballance the pressure of so small a Cylinder of Quick-silver, as then remain'd in the Tube.

Now (to satisfie our selves further, that the falling of the Quick-silver in the Tube to a determinate height, proceeds from the *Æquilibrium*, wherein it is at that height with the external Air, the one gravitating, the other pressing with equal force upon the subjacent Mercury) we Returned the Key and let in some new Air; upon which the Mercury immediatly began to ascend (or rather to be impell'd upwards) in the Tube, and continu'd ascending, till having Return'd the Key it immediatly rested at the height which it had then attain'd: And so, by Turning and Returning the Key, we did several times at pleasure impel it upwards, and check its ascent. And lastly, having given a free egress

egreſs at the Stop-cock to as much of the external Air as would come in, the Quick-ſilver was impell'd up almoſt to its firſt height: I ſay almoſt, becauſe it ſtopt near a quarter of an Inch beneath the Paper mark formerly mention'd; which we aſcrib'd to this, That there was (as is uſual in this Experiment) ſome little Particles of Air engag'd among thoſe of the Quick-ſilver; which Particles, upon the deſcent of the Quick ſilver, did manifeſtly riſe up in Bubbles towards the top of the Tube, and by their preſſure, as well as by leſſening the Cylinder by as much room as they formerly took up in it, hinder'd the Quick-ſilver from regaining its firſt height.

This Experiment was a few days after repeated in the preſence of thoſe excellent and deſervedly Famous Mathematick Profeſſors, Dr. *Wallis*, Dr. *Ward*, and Mr. *Wren*, who were pleaſed to Honor it with their Preſence: And whom I name, both as juſtly counting it an Honor to be known to them, and as being glad of ſuch Judicious and illuſtrious Witneſſes of our Experiment; and 'twas by their gueſs that the top of the Quick-ſilver in the Tube was defin'd to be brought within an Inch

of the surface of that in the Vessel.

And here, for the Illustration of the foregoing Experiment, it will not be amiss to mention some other particulars relating to it.

First then, When we endeavor'd to make the Experiment with the Tube clos'd at one end with *Diachylon* instead of an Hermetical Seal; we perceiv'd, that upon the drawing of some of the Air out of the Receiver, the Mercury did indeed begin to fall, but continu'd afterwards to subside, though we did not continue pumping. Whence it appear'd, that though the *Diachylon* that stopt the end of the Tube were so thick and strong, that the external Air could not press it in (as experience taught us that it would have done, if there had been but little of it) yet the subt'ler parts of it were able (though slowly) to insinuate themselves through the very body of the Plaister, which it seems was of so close a Texture, as that which we mention'd our selves to have successfully made use of in the Experiment *De Vacuo* some years ago. So that now we begin to suspect, that perhaps one Reason, why we cannot perfectly pump out the Air, may be, that when the Vessel

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is almoſt empty, ſome of the ſubtler parts of the external Air may, by the preſſure of the Atmoſphere, be ſtrain'd through the very body of the *Diachylon* into the Receiver. But this is onely conjecture:

Another Circumſtance of our Experiment was this, That, if (when the Quick-ſilver in the Tube was fallen low) too much ingreſs were, at the hole of the Stop-cock, ſuddenly permitted to the external Air; it would ruſh in with that violence, and beat ſo forcibly upon the ſurface of the ſubjacent Quick-ſilver, that it would impel it up into the Tube rudely enough to endanger the breaking of the Glaſs.

We formerly mention'd, that the Quick-ſilver did not in its deſcent fall as much at a time after the two or three firſt exſuctions of the Air, as at the beginning: For, having mark'd its ſeveral Stages upon the Tube, we found, that at the firſt ſuck it deſcended an Inch and $\frac{3}{8}$, and at the ſecond an Inch and $\frac{1}{8}$; and when the Veſſel was almoſt empty'd, it would ſcarce at one exſuction be drawn down above the breadth of a Barly-corn. And indeed we found it very difficult to meaſure in what

proportion these decrements of the Mercurial Cylinder did proceed: partly because (as we have already intimated) the Quick-silver was soon drawn below the top of the Receiver; and partly because, upon its descent at each exsuction, it would immediatly reascend a little upwards; either by reason of the leaking of the Vessel at some imperceptible hole or other, or by reason of the motion of Restitution in the Air, which, being somewhat comprest by the fall as well as weight of the Quick-silver, would repell it a little upwards, and make it vibrate a little up and down, before they could reduce each other to such an *Æquilibrium* as both might rest in. But though we could not hitherto make observations accurate enough concerning the measures of the Quick-silver's descent, to reduce them into any *Hypothesis*, yet would we not discourage any from attempting it; since, if it could be reduc'd to a certainty, 'tis probable that the discovery would not be unuseful.

And, to illustrate this matter a little more, we will adde, That we made a shift to try the Experiment in one of our above mention'd small Receivers, not containing

a Quart;

a Quart; but that (agreeably to what we formerly observed) we found it as difficult to bring this to be quite empty as to evacuate the greater; the least external Air that could get in (and we could not possibly keep it all perfectly out) sufficing in so small a Vessel to display a considerable pressure upon the surface of the Mercury, and thereby hinder that in the Tube from falling to a level with it. But this is remarkable, that having two or three times try'd the Experiment in that small Vessel, upon the very first Cylinder of Air that was drawn out of the Receiver, the Mercury fell in the Tube 18 Inches and a half, and at another 19 Inches and a half.

But, on this occasion, I hold it not unfit to give Your Lordship notice that I hop'd, from the descent of the Quick-silver in the Tube upon the first suck, to derive this advantage: that I should thence be enabled to give a near guess at the proportion of force betwixt the pressure of the Air (according to its various states, as to Density and Rarefaction) and the gravity of Quick-silver, then hitherto has been done. For in our Experiment there are diverse things given, that may be made use of towards such a discovery.

For first we may know the capacity of the Receiver wherein the Experiment is made, since, by filling it with water, we may easily compute how many Quarts, or Measures of any other denomination, it contains of Air; which Air, when shut up in the Vessel, may be suppos'd to have a pressure equal to that of the Atmosphere; since it is able to keep the Quick-silver in the Tube from falling any lower then it did in the free and open Air. Next here is given us the capacity of the brass Cylinder empty'd by the drawing down of the Sucker (its bore and height being mention'd in the description of our Pump) whereby we may come to know how much of the Air contain'd in the Receiver is drawn out at the first suck. And we may also easily define, either in weight or cubick measures the Cylinder of Quick-silver that answers to the Cylinder of Air lately mention'd (that Merculiall Cylinder being in our Engine computable by deducting from the entire altitude of that Cylinder of Quick-silver, the altitude at which it rests upon the first exsuction.) But though, if this Experiment were very watchfully try'd in Vessels of several sizes, and the

various deſcents of the Quick-ſilver compar'd among themſelves, 'tis not improbable that ſome ſuch thing as we hop'd for may thereby be diſcover'd. Yet becauſe not onely the ſolid contents of as much of the Glaſs-tube as remains within the concave ſurface of the Receiver, and (which is more difficult) the varying contents of the Veſſel containing the Mercury, and of as much of the Mercury it ſelf as is not in the Tube, muſt be deducted out of the capacity of the Receiver; but there muſt alſo an allowance be made for this, that the Cylinder that is empty'd by the drawing down of the Sucker, and comes to be fill'd upon the letting of the Air out of the Receiver into it, is not ſo repleniſh'd with Air as the Receiver it ſelf at firſt was: becauſe there paſſes no more Air out of the Receiver into the Cylinder, then is requiſite to reduce the Air in the cavity of the Cylinder, and in that of the Receiver to the ſame meaſure of dilatation: Becauſe of theſe (I ſay) and ſome other difficulties that require more skill in Mathematicks then I pretend to, and much more leaſure then my preſent occaſions would allow me, I was willing to refer the nicer conſideration of this matter to ſome

of

of our Learned and Acurate Mathematicians, thinking it enough for me to have given the Hint already ſuggeſted.

For further confirmation of what hath been delivered, we likewiſe tryed the Experiment in a Tube of leſs then two foot long: and, when there was ſo much Air drawn out of the Veſſel, that the remaining Air was not able to counterballance the Mercurial Cylinder, the Quick-ſilver in the Tube ſubſided ſo viſibly, that (the Experiment being try'd in the little Veſſel lately mention'd) at the firſt ſuck it fell above a ſpan, and was afterwards drawn lower and lower for a little while; and the external Air being let in upon it, impell'd it up again almoſt to the top of the Tube: So little matters it how heavy or light the Cylinder of Quick ſilver to ſubſide is, provided its gravity overpower the preſſure of as much external Air as bears upon the ſurface of that Mercury into which it is to fall.

Laſtly we alſo obſerv'd, That if (when the Mercury in the Tube had been drawn down, and by an Ingreſs permitted to the external Air, impell'd up again to its former height) there were ſome more Air thruſt up by the help of the Pump into

the Receiver, the Quick-ſilver in the Tube would aſcend much above the wonted height of 27 digits, and immediatly upon the letting out of that Air would fall again to the height it reſted at before.

Your Lordſhip will here perhaps expect, that as thoſe who have treated of the *Torricellian* Experiment, have for the moſt part maintaind the Affirmative, or the Negative of that famous Queſtion, Whether or no that Noble Experiment infer a *Vacuum?* ſo I ſhould on this occaſion interpoſe my Opinion touching that Controverſie, or at leaſt declare whether or no, in our Engine, the exſuction of the Air do prove the place deſerted by the Air ſuck'd out, to be truly empty, that is, devoid of all Corporeal Subſtance. But beſides that, I have neither the leiſure, nor the ability, to enter into a ſolemn Debate of ſo nice a Queſtion; Your Lordſhip may, if you think it worth the trouble, in the Dialogues not long ſince referr'd to, finde the Difficulties on both ſides repreſented; which then made me yield but a very wavering aſſent to either of the parties contending about the Queſtion: Nor dare I yet take upon me to determine ſo difficult a Controverſie.

For

For on the one ſide it appears, that notwithſtanding the exſuction of the Air, our Receiver may not be deſtitute of all Bodies, ſince any thing placed in it, may be ſeen there; which would not be, if it were not pervious to thoſe Beams of Light which rebounding from the ſeen Object to our eyes, affect us with the ſenſe of it: And that either theſe Beams are Corporeal Emanations from ſome lucid body, or elſe at leaſt the light they convey doth reſult from the brisk Motion of ſome ſubtle Matter, I could, if I miſtake not, ſufficiently manifeſt out of the Dialogues above-mention'd, if I thought your Lordſhip could ſeriouſly imagine that Light could be convey'd without, at leaſt, having (if I may ſo ſpeak) a Body for its Vehicle.

By the ſixteenth Experiment, it alſo appears that the cloſeneſs of our Receiver hinders it not from admitting the Effluvia of the Load-ſtone; which makes it very probable that it alſo freely admits the Magnetical ſteams of the Earth; concerning which, we have in another Treatiſe endeavour'd to manifeſt that numbers of them do always permeate our Air.

But on the other ſide it may be ſaid, That

That as for the ſubtle Matter which makes the Objects encloſed in our evacuated Receiver, viſible, and the Magnetical Effluvia of the Earth that may be preſum'd to paſs thorow it, though we ſhould grant our Veſſel not to be quite devoyd of them, yet we cannot ſo reaſonably affirm it to be repleniſh'd with them, as we may ſuppoſe, that if they were gather'd together into one place without Intervals between them, they would fill but a ſmall part of the whole Receiver. As in the thirteenth Experiment, a piece of Match was inconſiderable for its bulk, whileſt its parts lay cloſe together, that afterwards (when the Fire had ſcatter'd them into ſmoke) ſeem'd to repleniſh all the Veſſel. For (as elſewhere our Experiments have demonſtrated) both Light and the Effluvia of the Load-ſtone, may be readily admitted into a Glaſs, Hermetically ſeal'd, though before their Admiſſion, as full of Air as hollow Bodies here below are wont to be, ſo that upon the exſuction of the Air, the large ſpace deſerted by it, may remain empty, notwithſtanding the pretence of thoſe ſubtle Corpuſcles, by which Lucid and Magnetical Bodies produce their effects.

And

And as for the Allegations above mention'd, they seem to prove but that the Receiver devoy'd of Air, *May* be replenish'd with some such Etherial Matter, as some Modern Naturalists write of; but not that it really *is* so. And indeed to me it yet seems, that as to those spaces which the *Vacuists* would have to be empty, because they are manifestly devoid of Air; and all grosser Bodies, the *Plenists* (if I may so call them) do not prove that such spaces are replenish'd with such a subtle Matter as they speak of, by any sensible effects, or operations of it (of which divers new Tryals purposely made, have not yet shown me any) but onely conclude that there must be such a Body, because there cannot be a Void. And the reason why there cannot be a Void, being by them taken, not from any Experiments, or *Phænomena* of Nature, that clearly and particularly prove their *Hypothesis*, but from their notion of a Body, whose Nature, according to them, consisting onely in extension (which indeed seems the property most essential to, because inseparable from a Body) to say a space devoid of Body, is to speak in the School-mens Phrase, a Contradiction *in Adjecto*: This

reason, I say, being thus desum'd, seems to make the Controversie about a *Vacuum*, rather a Metaphysical, then a Physiological Question; which therefore we shall here no longer debate, finding it very difficult either to satisfie Naturalists with this Cartesian Notion of a Body, or to manifest wherein it is erroneous, and substitute a better in its stead.

But though we are unwilling to examine any further the Inferences wont to be made from the *Torricellian* Experiment, yet we think it not impertinent to present Your Lordship with a couple of Advertisements concerning it.

First, then if in trying the Experiment here or elsewhere, you make use of the English measures that Mathematicians and Tradesmen are here wont to imploy, You will, unless you be forewarn'd of it, be apt to suspect that those that have written of the Experiment have been mistaken. For whereas men are wont generally to talk of the Quick-silver's remaining suspended at the heighth of between six or seven and twenty Inches; we commonly observ'd, when divers years since we first were sollicitous about this Experiment, that the Quick-silver in the Tube

rested at about 29 Inches & an half above the surface of the Restagnant Quick-silver in the Vessel, which did at first both amaze and perplex us, because though we held it not improbable that the difference of the grosser English Air, and that of *Italy* and *France*, might keep the Quick-silver from falling quite as low in this colder, as in those warmer Climates; yet we could not believe that that difference in the Air should alone be able to make so great a one in the heights of the Mercurial Cylinders; and accordingly upon enquiry we found, that though the various density of the Air be not to be over-look'd in this Experiment, yet the main Reason why we found the Cylinder of Mercury to consist of so many Inches, was this, That our English Inches are somewhat inferior in length to the digits made use of in Forein Parts, by the Writers of the Experiment.

The next thing I desire Your Lordship to take notice of, is, That the heigth of the Mercurial Cylinder is not wont to be foũd altogether so great as really it might prove, by reason of the negligence or incogitancy of most that make the Experiment. For often times upon the opening

of the inverted Tube into the Veſſell'd Mercury, you may obſerve a bubble of Air to aſcend from the bottom of the Tube through the ſubſiding Quick-ſilver to the top; and almoſt always you may, if you look narrowly, take notice of a multitude of ſmall bubbles all along the inſide of the Tube betwixt the Quick-ſilver & the glaſs:(not now to mention the Particles of Air that lye conceal'd in the very Body of the Mercury) Many of which, upon the Quick-ſilvers forſaking the upper part of the Tube, do break into that deſerted ſpace where they finde little or no reſiſtance to their expanding of themſelves. Whether this be the reaſon that upon the Application of warm Bodies to the emptyed part of the Tube, the ſubjacent Mercury would be depreſſ'd ſomewhat lower, we ſhall not determine; though it ſeem very probable, eſpecially ſince we found that upon the application of Linnen cloaths dipped in Water, to the ſame part of the Tube, the Quick-ſilver would ſomewhat aſcend, as if the cold had condenſ'd the Impriſon'd Air, that preſſ'd upon it, into a leſſer room. But that the deſerted ſpace is not wont to be totally devoid of Air, we were induc'd

to think by ſeveral Circumſtances. For when an eminent Mathematician, and excellent Experimenter, had taken great pains and ſpent much time in accuratly filling up a Tube of Mercury, we found that yet there remain'd ſtore of inconſpicuous bubbles, by inverting the Tube, letting the Quick-ſilver fall to its wonted heighth; and by approaching (by degrees) a red hot Iron to the out-ſide of the Tube, over againſt the upper part of the Mercurial Cylinder, for hereby the little unheeded bubbles, being mightily expanded, aſcended in ſuch numbers, and ſo faſt to the deſerted ſpace, that the upper part of the Quick-ſilver ſeem'd, to our wonder, to boyl. We further obſerv'd, That in the tryals of the *Torricellian* Experiment we have ſeen made by others, and (one excepted) all our own, we never found that upon the inclining of the Tube the Quick-ſilver would fully reach to the very top of the ſeal'd end: which argued, that there was ſome Air retreated thither that kept the Mercury out of the unrepleniſh'd ſpace.

If Your Lordſhip ſhould now demand what are the beſt expedients to hinder the intruſion of the Air in this Experiment; we

we must answer, That of those which are easily intelligible without ocular demonstration, we can at present suggest upon our own tryals no better then these. First, at the open end of the Tube the Glass must not onely be made as even at the edges as you can, but it is very convenient (especially if the Tube be large) that the bottom be every way bent inwards, that so the Orifice, not much exceeding a quarter of an Inch in Diameter, may be the more easily and exactly stopp'd by the Experimenter's finger; between which and the Quick-silver, that there may be no Air intercepted (as very often it happens that there is) it is requisite that the Tube be fill'd as full as possibly it can be, that the finger which is to stop it, pressing upon the accumulated and protuberant Mercury, may rather throw down some, then not finde enough exactly to keep out the Air. It is also an useful and compendious way not to fill the Tube at first quite ful of Mercury, but to leave near the top about a qnarter of an Inch empty; for if you then stop the open end with your finger, and invert the Tube that quarter of an Inch of Air will ascend in a great bubble to the top, and in its passage thi-

ther, will gather up all the little bubbles, and unite them with it ſelf into one great one, ſo that if by reinverting the Tube you let that bubble return to the open end of it, you will have a much cloſer Mercurial Cylinder then before, and need but to adde a very little Quick-ſilver more to fill up the Tube exactly. And laſtly, as for thoſe leſſer and inconſpicuous parcels of Air which cannot this way be gleaned up, You may endeavor before you invert the Tube, to free the Quick-ſilver from them by ſhaking the Tube, and gently knocking on the out-ſide of it, after every little parcel of Quick-ſilver which you pour in; and afterwards, by forcing the ſmall latitant bubbles of Air to diſcloſe themſelves and break, by imploying a hot Iron in ſuch manner as we lately mention'd. I remember that by carefully filling the Tube, though yet it were not quite free from Air, we have made the Mercurial Cylinder reach to 30 Inches and above an eighth, and this in a very ſhort Tube: which we therefore mention, becauſe we have found, by experience, that in ſhort Tubes a little Air is more prejudicial to the Experiment then in long ones, where the Air having more room to expand it

ſelf,

ſelf, does leſs potently preſs upon the ſubjacent Mercury.

And ſince we are fallen upon the conſideration of the Altitude of the Mercurial Cylinder, I muſt not conceal from Your Lordſhip an Experiment relating thereunto, which perhaps will ſet both You and many of your Friends the *Virtuoſi* a thinking; and, by diſcloſing ſome things about the Air or Atmoſphere that have ſcarce hitherto been taken notice of, may afford you ſome hints conducive to a further diſcovery of the ſubject of this Epiſtle.

WE took a Glaſs Tube, which, though it were not much above three Foot long, we made choice of becauſe it was of a more then ordinarily even thickneſs. This we fill'd with Mercury, though not with as much care as we could, yet with ſomewhat more then is wont to be uſed in making the *Torricellian* Experiment. Then, having according to the manner inverted the Tube, and open'd the mouth of it beneath the ſurface of ſome other Quick-ſilver, that in the Tube fell down to the wonted heigth, leaving, *Experiment* 18.

as is usual, some little Particles of Air in the space it deserted, as we ghest by observing, that upon the Application of hot Bodies to the upper part of the Tube, the Quick-silver would be a little depress'd. Lastly, having put both the Tube and the Vessel it lean'd on into a convenient Wooden Frame, to keep them from mischances: we plac'd that Frame in a Window within my Bed-chamber, that I might both keep the Mercury from being stirr'd, and have opportunity to watch from time to time the *Phænomena* it was to exhibit. For the better discovery of which, when the Quick-silver both in the Tube and subjacent Vessel was perfectly at rest, we took notice, by a mark made on the outside of the Glass, how high the included Liquor then reach'd.

During several Weeks that the Tube was kept in that Window (which was very rarely open'd) I had the opportunity to observe, that the Quick-silver did sometimes faintly imitate the Liquor of a Weather-glass, subsiding a little in warm, and rising a little in cold Weather, which we ascribed to the greater or lesser pressure of that little Air that remain'd at the top of the Tube, expanded or condens'd by

the heat or cold that affected the ambient Air. But that which I was chiefly careful to obſerve, was this, That oftentimes the Quick-ſilver did riſe and fall in the Tube, and that very notably, without conforming it ſelf to what is uſual in Weather-glaſſes, whoſe Air is at the top, nay quite contrary thereunto: for ſometimes I obſerv'd it in very cold weather (ſuch as this Winter has already afforded us good ſtore of) to fall down much lower then at other times, when by reaſon of the abſence of both Froſt, Snow, and ſharp Winds, the Air was comparatively much warmer. And I further obſerv'd, That ſometimes the Quick-ſilver would for ſome days together reſt almoſt at the ſame height; and at other times again it would in the compaſs of the ſame day conſiderably vary its altitude, though there appear'd no change either in the Air abroad, or in the temper of the Air within the Room (wherein was conſtantly kept a good Fire) nor in any thing elſe, to which either I, or ſome eminently Learned Men whom I then acquainted with the Experiment, could reaſonably impute ſuch a change: Eſpecially conſidering that the ſpace wherein the Mercury wandred up and down, within about five Weeks, amounted to full two Inches, of which we

found

found by our ſeveral marks whereby we had taken notice of its ſeveral removes, that it had deſcẽded about $\frac{9}{16}$ of an Inch from the place where it firſt ſetled, & the other Inch and $\frac{7}{16}$ it had aſcended. And it ſeems probable that the height of the Mercurial Cylinder would have varied yet more, if the Experiment had been made in the open Air and in a long Tube, where the Particles of the Impriſon'd Air, by having more room to diſplay themſelves in, might not have had ſo ſtrong a Spring to work upon the Quick-ſilver with. But for want both of time and of a competent quantity of Mercury (which was not to be procur'd where we then happen'd to be) we were unable to make any further tryals: which therefore chiefly troubled us, becauſe we would gladly have try'd an ingenious Experiment which was ſuggeſted unto us by that excellent Mathematician Mr. *Wren*, who being invited to name any thing he would have us try touching the preſſure of the Air, deſired us to obſerve whether or no the Quick-ſilver in a long Tube would not a little vary its height according to the Tides, eſpecially about the New and Full Moon, about which times Mariners obſerve thoſe great Flowings and Ebbs of the Sea, that they call the

Spring-

Spring-Tides. For he ſagaciouſly and plauſibly conjectur'd that ſuch obſervations accurately made, would diſcover the truth or erroneouſneſs of the *Carteſian Hypotheſis* concerning the Ebbing and Flowing of the Sea: which *Des Cartes* aſcribes to the greater preſſure made upon the Air by the Moon, and the Intercurrent Ethereal Subſtance at certain times (of the Day, and of the Lunary Moneth) then at others. But in regard we found the Quick-ſilver in the Tube to move up and down ſo uncertainly, by reaſon, as it ſeems, of accidental mutation in the Air; I ſomewhat doubt whether we ſhall finde the Altitude of the Quick-ſilver to vary as regularly as the Experiment is ingeniouſly propoſ'd. The ſucceſs we ſhall (God permitting us to make tryal of it) acquaint Your Lordſhip with; and in the mean time take notice, that when we had occaſion to take the Tube out of the Frame (after it had ſtaid there part of *November* and part of *December*) a good Fire being then in the room, becauſe it was a Snowy day, we found the Quick-ſilver in the Tube to be above the upper ſurface of the ſubjacent Mercury 29 Inches three quarters,

If Your Lordſhip ſhould now ask me what are the true cauſes of this varying altitude of the Mercurial Cylinder; I ſhould not undertake to anſwer ſo difficult a queſtion, and ſhould venter to ſay no more, then that among divers poſſible cauſes to which it may be aſcribed, it would not be, perhaps, abſurd to reckon theſe that follow.

Firſt then we may conſider, that the Air in the upper part of the Tube is much more rarified, and therefore more weak then the external Air, as may appear by this among other things, That upon the inclining of the Tube the Quick-ſilver will readily aſcend almoſt to the very top of it, and ſo take up eight or nine tenth parts, and perhaps more of that ſpace which it deſerted before: which would not happen if that whole ſpace had been full of unrarified Air, ſince that (as tryal may eaſily ſatisfie you) would not have ſuffer'd it ſelf to be thruſt into ſo narrow a room by ſo weak a preſſure. So that although in our Tube when the included Air was heated, the Quick-ſilver was ſomewhat depreſſ'd: Yet there is this difference betwixt ſuch a Tube and common Weather-Glaſſes, that in theſe the included and the

ambient Air are in an *Æquilibrium* as to pressure, and the weight of the Water that keeps them separate is scarce considerable. Whereas in such a Tube as we are speaking of, the Air within is very much more dilated then that without: and 'tis not so much the spring or resistance of the included Air, as the weight of the Mercurial Cylinder it self that hinders the Quick-silver from ascending higher; for if we should suppose that deserted part of the Tube perfectly devoid of Air, yet would the Quick-silver rise but a little higher in it, and be far from filling it, in regard the outward Air would not be able to impel up such a weight much higher: whereas it may, by our former Experiments appear, that if all the Air in the upper part of a Weather-Glass were away, the Water would be impell'd up to the very top of it, though the Pipe were above thirty Foot long.

We may next consider, that this rarified Air at the upper part of our Tube being exactly shut up betwixt the Glass and the Quick-silver, it was scarce subject to any discernable alterations, save those it receiv'd from heat and cold.

And

And we may further consider that yet the external Air or Atmosphere is subject to many alterations, besides them that proceed from either of those Qualities.

For the Experiment that occasion'd this Discourse, seems to make it probable enough that there may be strange Ebbings and Flowings, as it were, in the Atmosphere; or at least, that it may admit great and sudden Mutations, either as to its Altitude or its Density, from causes, as well unknown to us, as the effects are unheeded by us. And that You may not think that there is nothing in Nature but our Experiment that agrees with this our conjecture, we might put Your Lordship in minde of the Pains and Aches that are often complain'd of by those that have had great Wounds or Bruises, and that doe presage great Mutations in the Air oftentimes, whilst to strong and healthy Persons no sign of any such thing appears. And that is also very memorable to this purpose, which I remember I have somewhere read in a Book of the Ingenious *Kircherus*, who giving a pertinent admonition concerning the various refractions that may happen in the Air, relates, That during

during his ſtay in *Malta*, he often ſaw Mount *Ætna*, though the next day, notwithſtanding its being extreamly clear, he could not ſee it; adding, that *Vintemillius*, a very Learned Perſon, did oftentimes, from a Hill he names, behold the whole Iſland he calls *Luprica* protuberant above the Sea, though at other times, notwithſtanding a clear Sky, he could not ſee it. And though perhaps this may be in part aſcribed to the various light & poſition of the ſun, or to the various diſpoſition of the Spectators eye, or peradventure to ſome other cauſe; yet the moſt probable cauſe ſeems to be the differing Denſity of the Air, occaſion'd by Exhalations capable to increaſe the refraction, and conſequently bring Beams to the Eye, which otherwiſe would not fall on it. We have likewiſe in another Treatiſe mention'd our having often obſerv'd with Teleſcopes a plenty of Steams in the Air, which without ſuch a help would not be taken notice of; and which as they were not at all times to be ſeen even through a Teleſcope, ſo they did ſometimes, eſpecially after a ſhower of Rain, haſtily diſappear: and when we have viſited thoſe places that abound with Mines, we have ſeveral times been told

by

by the Diggers, that even when the Sky ſeem'd clear, there would not ſeldom ſuddenly ariſe, and ſometimes long continue, a certain Steam (which they uſually call a damp) ſo groſs and thick, that it would oftentimes put out their very Candles, if they did not ſeaſonably prevent it. And I think it will eaſily be granted, that the aſcenſion of ſuch Steams into this or that part of the Air, and their mixing with it, are very like to thicken it; as on the other ſide either heat or the ſudden condenſation of the Air in another part of the Atmoſphere (to mention now no other cauſes) are capable of rarifying it.

Nor will it very much import the main ſcope of our Diſcourſe, whether it be ſuppoſ'd that the copious Steams the earth ſends into the air, thicken that part of the Atmoſphere that receives them, and make it more heavy: Or that ſometimes the Fumes may aſcend with ſuch celerity, that though the Air be thicken'd yet they rather diminiſh then encreaſe its gravitation, in regard that the quickneſs of their aſcent, not onely keeps them from gravitating themſelves, but may hinder the preſſing downwards of many Aërial Corpuſcles that they meet with in

their

their way upwards. This, I say, is of no great importance to our present Discourse, since either way the Terrestrial Steam may here and there considerably alter the gravity or pressure of the Atmosphere.

Your Lordship may also be pleased to remember, That by our seventeenth Experiment it appear'd that as when the Air in the Receiver was expanded more then ordinarily, the Quick-silver in the Tube did proportionably subside; so when the Air in the same Receiver was a little more then ordinarily compress'd, it did impell up the Quick-silver in the Tube above the wonted height of betwixt six and seven and twenty digits.

And if to these things we annex, that for ought we can finde by tryals purposely made, the degree of rarity or density of the Air, shut up into our Receiver, does not sensibly alter its temperature as to cold or heat. It will not, I hope, appear absurd to conceive, That since the Air, included in the Tube, could but very faintly hinder the ascent of the Quick-silver, or press it downwards, since too that included Air could scarce immediately receive any sensible alteration, save either by heat

 or

or cold. And ſince alſo that according to the bare denſity or rarity of the Air incumbent on the ſubjacent Quick-ſilver in the Veſſel, that in the Tube was impell'd more or leſs high; ſuch changes happening in the neighboring part of the outward Air, either by the aſcenſion of groſs or copious exhalations, or by any other cauſe (of which there may be divers) as were capable to make conſiderable alterations in the conſiſtence of the Air, as to rarity and denſity, *may* be able proportionably to alter the heighth of the Quick-ſilver: I rather ſay, that ſuch alterations *may* be, then that they *are* the cauſes of our *Phænomenon*, becauſe I think it ſufficient, if I have propoſ'd conjectures not altogether irrational about a new Myſtery of Nature, touching which, the chief thing I pretend to, is to give occaſion to the Curious to inquire further into it then I have been yet able to do.

Experiment 19. THe ſame Reaſon that mov'd us to conclude, that by the drawing of the Air out of the Receiver, the Mercury would deſcend in a Tube ſhorter then ſix and twenty digits, induc'd us alſo to expect,

pect, that by the ſame means Water might be brought to ſubſide in Glaſs Tubes of a moderate length, though by the noble Experiment, ſaid to have been accurately made in *France* by Monſieur *Paſchal*, we are informed that a Tube of no leſs then about two and thirty Foot, was found requiſite to make the Experiment *De Vacuo* ſucceed with Water inſtead of Quick-ſilver: ſo tall a Cylinder of that lighter Liquor, being, it ſeems, requiſite to equal the weight of a Mercurial Cylinder of ſix or ſeven and twenty digits, and ſurmount the preſſure of the Atmoſphere.

We took then a Tube of Glaſs, Hermetically ſeal'd at one end, of about four foot in length, and not very ſlender: This at the open end we fill'd with common Water, and then ſtopt that end till we had inverted the Tube, and open'd it beneath the ſurface of a quantity of the like Water, contain'd in a ſomewhat deep and ſlender Veſſel. This Veſſel, with the Tube in it, was let down into the Receiver, and the Receiver being cloſ'd up after the accuſtom'd manner, the Pump was ſet awork.

As much of the event as concerns our present purpose, was this, That till a considerable part of the Air was drawn out of the Receiver, the Tube continu'd top-full of Water as when it was put in, it being requisite that a great part of the Air formerly contain'd in the Receiver, should be drawn out, to bring the remaining Air to an *Æquilibrium* with so short and light a Cylinder of Water. But when once the Water began to fall in the Tube, then each exsuction of Air made it descend a little lower, though nothing near so much as the Quick-silver at the beginning did in the Experiment formerly mention'd. Nor did there appear so much inequality in the spaces transmitted by the Water in its descent, as there did in those observ'd in the fall of the Quick-silver, of which the cause will scarce seem abstruse to him that shall duly reflect upon what has been already deliver'd, And whereas we drew down the Quick-silver in the Tube so far as to bring it within an Inch of the surface of the other Quick-silver into which it was to fall; the lowest we were able to draw down the Water was, by our conjecture, to about a Foot

or more above the ſurface of that in the Veſſel; of which I know not whether it will be needful to aſſign ſo obvious a cauſe as that, though the little Air remaining in the Receiver could not hinder a Cylinder of above an Inch high of Quick-ſilver from ſubſiding; yet it might very well be able, by its preſſure, to countervail the weight of a Cylinder of a Foot long or more, of a Liquor ſo much leſs ponderous then Quick-ſilver, as Water is. And in fine, to conclude our Experiment, when the Water was drawn down thus low, we found, that by letting in the outward Air, it might be immediately impell'd up again to the higher parts of the Tube.

We will adde no more concerning this Experiment, ſave that having try'd it in one of our ſmall Receivers, we obſerv'd, That upon the firſt exſuction of the Air the Water did uſually ſubſide divers Inches, and at the ſecond (exſuction) fall down much lower, ſubſiding ſometimes near two Foot; as alſo that upon the letting in of the Air from without, the Water was impell'd up with very great celerity.

Experiment 20. THat the Air has a notable Elastical power (whencesoever that proceeds) we have, I suppose, abundantly evinc'd, and it begins to be acknowledg'd by the eminentest Modern Naturalists. But whether or no there be in Water so much as a languid one, seems hitherto to have been scarce consider'd, nor has been yet, for ought I know, determin'd either way by any Writer, which invited us to make the following Experiment.

There was taken a great Glass-bubble, with a long neck; (such as Chymists are wont to call a Philosophical Egg) which being fill'd with common Water till the Liquor reach'd about a span above the bubble, and a piece of Paper being there pasted on, was put unstop'd into the Receiver, and then the Air was suck'd out after the wonted manner. The event was this, That a considerable part of the Air, pent up in the Receiver, was drawn out before we discern'd any expansion of the Water; but, continuing the labor of pumping, the Water manifestly began to ascend in the stem of the Glass, and divers bubbles loosening themselves from the

the lower parts of the Veſſel, made their way through the Body of the Water, to the top of it, and there brake into the Receiver: And after the Water once appear'd to ſwell, then at each time the Stopcock was turn'd to let out the air from the Receiver into the Pump, the Water in the Neck of the Glaſs did ſuddenly riſe about the breadth of a Barly-corn in the Neck of the Glaſs, and ſo attain'd, by degrees, to a conſiderable height above the mark formerly mention'd. And at length (to make the expanſion of the Water more evident) the outward Air was ſuddenly let in, and the Water immediately ſubſided and deſerted all the ſpace it had newly gain'd in the Glaſs.

And, on this occaſion, it will not perhaps be amiſs to acquaint Your Lordſhip here (though we have already mention'd it in another Paper, to another purpoſe) with another Expedient that we made uſe of two or three years ago, to try whether or no Water had a Spring in it. About that time then, That Great and Learned Promoter of Experimental Philoſophy Dr. *Wilkins*, doing me the Honor to come himſelf, and bring ſome of his inquiſitive Friends to my Lodging, we

there had in readineſs a round and hollow Veſſel of Pewter, great enough to contain two pounds of Water, and exactly cloſe every where, but at one little hole where it was to be fill'd; then partly by ſucking out the Air, and partly by injecting Water with a Syringe, it was (not without ſome difficulty) fill'd up to the top; and that hole being plac'd directly upwards, there was a little more Water leiſurely forc'd in by the Syringe. Upon which, though the Veſſel were permitted to reſt, and the hole kept in its former poſture, yet the compreſs'd Water leiſurely ſwell'd above the Orifice of the hole, and divers drops ran over along the ſides of the Veſſel. After this, we cauſ'd a skilful Pewterer (who had made the Globe) to cloſe it up in our preſence with Soder ſo exquiſitely, that none ſuſpected there was any thing left in it beſides Water. And laſtly, the Veſſel thus ſoder'd up, was warily and often ſtruck in divers places with a Wooden Mallet, and thereby was manifeſtly compreſs'd, whereby the incloſed Water was crouded into leſs room then it had before: And thereupon when we took a Needle, and with it and the Mallet perforated the Veſſel, and drew out the

Needle

Needle again; the Water (but in a very ſlender Stream) was ſuddenly thrown after it into the Air, to the height of two or three Feet. As for the other *Phænomena* of this Experiment, ſince they belong not to our preſent purpoſe, and are partly mention'd in another of our Papers, we ſhall, inſtead of recording them here, give this Advertiſement: That as evidently as this Experiment, and that made in our Receiver, ſeem to prove a power in the Water to expand and reſtore it ſelf after compreſſion; yet for a reaſon to be met with ere long, I judged it not ſafe to inſer that Concluſion from theſe Premiſes, till I had made ſome of the following tryals, to the mention of which I will therefore haſten.

TO diſcover whether the Expanſion of the Water really proceeded from an Elaſtical power in the parts of the Water it ſelf, we thought it requiſite to try two things: The one, Whether or no the Atmoſphere gravitates upon Bodies under Water; and the other, Whether in caſe it do gravitate, the Intumeſcence of the Water may not be aſcribed to ſome *Experiment 21.*

ſub-

substance subtler then it self, residing in it. In order to the satisfying my self about the first of these, I intended to let down into the Receiver a Vessel of Water, wherein should be immers'd a very small oyl'd Bladder, almost devoid of Air, but strongly ty'd up at the Neck with a string, and detain'd a little under Water by such a weight fasten'd to that string, as should just be able to keep the Bladder from swimming, and no more. For I suppos'd, that if when all things were thus order'd, the Receiver were empty'd, in case there were any such pressure of the Atmosphere upon Water, as I was inclin'd to believe, the Air within the Bladder, being upon the exsuction of the Air within the Receiver, freed from that pressure, and being press'd onely by the small weight of the incumbent Water, would considerably expand it self; but whil'st we were preparing Bladders for this Experiment, there occurr'd an easie way for the making at once both the Discoveries I desir'd.

We took then a Glass Viol, containing by ghess a pound and some ounces of Water; this we fill'd top full, and then we put into the Neck of it a Glass Pipe a pretty deal bigger then a Goose Quill, open

open at both ends, and of divers Inches in length: One end of this Pipe was ſo put into the Neck of the Viol, as to reach a little below it, and then was carefully cemented thereto that no Air might get into the Viol, nor no Water get out of it, otherwiſe then through the Pipe; and then the Pipe being warily fill'd, about half way up to the top, with more Water, and a mark being paſted over againſt the upper ſurface of the Liquor; the Viol thus fitted with the Pipe, was, by ſtrings let down into the Receiver, and according to the wonted manner exquiſitely cloſ'd up in it.

This done, we began to Pump out the Air, and when a pretty quantity of it had been drawn away, the Water in the Pipe began to riſe higher in the Pipe, at the ſides of which ſome little bubbles diſcover'd themſelves. After a little while longer, the Water ſtill ſwelling, there appear'd at the bottom of the Pipe a bubble about the bigneſs of a ſmall Pea, which aſcending through the Pipe to the top of the Water, ſtaid there awhile and then broke; but the Pump being nimbly ply'd, the expanſion of the Water ſo encreaſ'd, that quickly, getting up to the

top

top of the Pipe some drops of it began to run down along the out-side of it, which oblig'd us to forbear pumping awhile, and give the Water leave to subside within less then two Inches of the bottom of the Pipe. After this the Pump being again set at work, the bubbles began to ascend from the bottom of the Pipe, being not all of a size, but yet so big, that estimating one with another, they appear'd to be of the size of the smaller sort of Peas; and of these we reckon'd about sixty which came up one after another, besides store of smaller ones, of which we made no reckoning: And at length, growing weary of reckoning and pumping too (because we found, that in spight of all our pains and industry, some undiscern'd Leak or other in the Receiver hinder'd us from being able to empty it altogether) we thought fit to desist for that time. After tryal made of what operation the external Air, being let in upon the expanded Water, would have; and accordingly turning the Key to let in the Air, we saw, as we expected, that the Water in the Pipe in a moment fell down almost to the bottom of it.

Now

Now of this Experiment there are two or three Circumſtances yet to be mention'd, which are no leſs then thoſe already recited, pertinent to our preſent purpoſe.

In the firſt place then, when the greater part of the Air had been pump'd out of the Receiver, the riſing bubbles aſcended ſo very ſlowly in the Pipe, that their Progreſs was ſcarce diſcernable; which ſeem'd to proceed from this, That their bigneſs was ſuch, That they could not ſufficiently extend themſelves in the cavity of the Glaſs, without preſſing on both hands againſt the ſides of it, whereby they became of more difficult extruſion to the Water. And though it may ſeem ſtrange theſe bubbles ſhould be of any conſiderable bulk, ſince 'tis like they conſiſted of leſſer parcels of the Air lurking in the Water, then thoſe that were vigorous enough to make their way through long before them: yet they were commonly much larger then before, ſome of them being equal in quantity to four or five Peas: Whether this their increaſe of bulk proceeded from the greater decrement of the preſſure of the Air,

or

or from the Union of two or three of thoſe numerous bubbles which were then generated below the bottom of the Pipe, where we could not ſee what was done among them.

Another thing we noted in our bubbles was, That whereas in ordinary ones the Air, together with the thin film of Water that inveſts and detains, is wont to ſwell above the ſurface of the Water it ſwims on, and commonly to conſtitute Hemiſpherical Bodies with it, the little parcels of Air that came up after the Receiver was pretty well empty'd, did not make protuberant bubbles, but ſuch whoſe upper ſurface was either level with or beneath that of the Water, ſo that the upper ſurface being uſually ſomewhat convex, the leſs protuberant parts of it had a pretty quantity of Water remaining above them.

We alſo further obſerv'd, That whereas in the bubbles that firſt appear'd in the Pipe, the aſcending Air did, as in other common bubbles, make its way upwards, by dividing the Water through which it paſs'd, in thoſe bubbles that appear'd at the latter end of our Experiment, when the preſſure of the little external

ternal Air, remaining in the Receiver; was grown inconsiderable, the ascending parcels of Air having now little more then the weight of the incumbent Water to surmount, were able both so to expand themselves as to fill up that part of the Pipe which they pervaded, & by pressing every way against the sides of it, to lift upwards with them what Water they found above them, without letting any considerable quantity glide down along the sides of the Glass: So that sometimes we could see a bubble thrust on before it a whole Cylinder of Water of perhaps an Inch high, and carry it up to the top of the Pipe; though as we formerly noted, upon the letting in the external Air, these tumid bubbles suddenly relaps'd to their former inconspicuousness.

All these things laid together seem'd sufficiently to confirm that, which the consideration of the thing it self would easily enough perswade, namely, That the Air, and such like Bodies being under Water, may be press'd upon as well by the Atmosphere, as by the weight of the incumbent Water it self.

Hence likewise we may verifie what we observ'd at the close of the foregoing

Expe-

Experiment, namely, That from the ſole ſwelling of Water there recorded, it cannot be ſo ſafely concluded that Water, when freed from compreſſion, is endow'd with an Elaſtical power of expanding it ſelf, ſince thereby it appears that the Intumeſcence produc'd by that Experiment, may (at leaſt in great part) be aſcrib'd to the numerous little bubbles which are wont to be produc'd in Water, from which the preſſure of the Atmoſphere is in great meaſure taken off. So apt are we to be miſ-led, even by Experiments themſelves, into Miſtakes, when either we conſider not that moſt Effects may proceed from various Cauſes, or minde onely thoſe Circumſtances of our Experiment, which ſeem to comply with our preconceiv'd *Hypotheſis* or Conjectures.

And hence it ſeems alſo probable, that in the Pores or inviſible little receſſes of Water it ſelf there lie commonly interſperſ'd many parcels of either Air, or at leaſt ſomething Analogous thereunto, although ſo very ſmall that they have not been hitherto ſo much as ſuſpected to lurk there. But if it be demanded how it appears that there is interſperſ'd through the Body of Water any ſubſtance thinner

then

then it ſelf, and why that which produc'd the bubbles above mention'd ſhould not be reſolutely ſaid to be nothing elſe then a more active and ſpirituous part of the Water, we ſhall, in order to the Elucidation of this matter, ſubjoyn to what was formerly deliver'd the following Experiment.

Experiment 22.

VVE recited in our nineteenth Experiment, how by drawing moſt of the Air out of the Receiver, we made the Water ſubſide by degrees in a Glaſs not four Foot long: We ſhall now adde, that in the like Experiment made in ſuch a Tube, or a greater, it may be obſerv'd, That when the Water begins to fall, there will appear ſtore of bubbles faſten'd all along to the ſides of the Glaſs; of which bubbles, by the agitation of the Veſſel conſequent upon pumping, there'will ariſe good numbers to the top of the Water, and there break; and as the Cylinder of Water is brought to be lower and lower, ſo the bubbles will appear more numerous in that part of the Tube which the Water yet fills; and the nearer the ſurface of the Water, in its deſcent, approaches to theſe

bubbles, the greater they will grow, because having the leſs weight and preſſure upon them, the Expanſion of that Air which makes them, can be the leſs reſiſted by the preſſure of the incumbent Water and Air; as ſeems probable from hence, that upon the letting in a little external Air, thoſe bubbles immediately ſhrink.

It may indeed, as we lately intimated, be conjectur'd, that theſe bubbles proceed not ſo much from any Air pre-exiſtent in the Water, and lurking in the Pores of it, as from the more ſubtle parts of the Water it ſelf; which by the expanſion allow'd them upon the diminiſh'd preſſure of the ambient Bodies may generate ſuch bubbles. And indeed, I am not yet ſo well ſatisfied that bubbles may not (at leaſt ſometimes) have ſuch an Origination: but that which makes me ſuſpect that thoſe in our tryals contain'd real Air formerly latitant in the Pores of the Water, is this, That upon the inletting of the external Air, the Water was not again impell'd to the very top of the Tube whence it began to fall, but was ſtopt in its aſcent near an Inch beneath the top. And ſince, if the upper part of the Tube had been devoyd of any other

then

then ſuch Ethereal matter as was ſubtle enough freely to penetrate the pores of the Glaſs, the external Air would have been able to impel the Water to the top of a Tube ſeven or eight times as long as ours was; The *Phænomenon* under conſideration ſeem'd manifeſtly to argue that the many bubbles that broke at the top of the Water did contain a real Air, which, being collected into one place and hinder'd by the top of the Glaſs from receding, was able to withſtand the preſſure of the outward Air. As we ſee that if never ſo little Air remain in the Tube upon the making the Experiment *De Vacuo* with Quick-ſilver, no inclining of the Tube, though a long one, will enable a Man to impel the Mercury up to the very top, by reaſon (as we formerly noted) of the reſiſtance of the included Air, which will not be compreſſ'd beyond a certain degree.

But in order to a further Diſcovery what our bubbles were, we will, on this occaſion, inform Your Lordſhip that we try'd the XIX^th *Experiment* in one of our ſmall Receivers, and found, that upon the drawing down of the Water, ſo many bubbles diſcloſ'd themſelves and broke into the

upper part of the Tube, that having afterwards let in the external Air, the Water was not thereby impell'd to the top of the Tube (three Foot in length) within a little more then half an Inch. And whether or no it were Air that possess'd that space at the top of the Tube which was not fill'd with Water, we took this course to examine. We drew the second time the Air out of the Receiver, and found, that by reason of the body that possess'd the top of the Tube, we were able not onely to make the Water in the Tube fall to a level with the surface of the Water in the Vessel: But also (by plying the Pump a little longer) a great way beneath it: which since it could not well be ascrib'd to the bare subsiding of the Water by reason of its own weight, argued that the Water was depress'd by the Air: which was confirm'd by the Figure of the surface of the Water in the Tube, which was much more concave then that of Water in Tubes of that bigness uses to be. And this further tryal (to adde that upon the by) we made at the same time, That when the Water in the Pipe was drawn down almost as low as the Water without it, we observ'd, that (though we desisted

from

from pumping) by the bare application of a hand moderately warm to the desert-ed part of the Tube, the remaining Water would be speedily and notably depress'd. And having for a while held a kindled Coal to the outside of the Tube, (the Pump being still unimploy'd, because the Vessel chanced to hold extraordinarily well) the Air was by the heat so far expanded, that it quickly drave the Water to the bottom of the Tube, which was divers Inches beneath the surface of the ambient Water. Whereby it appears (by the same way by which we formerly measur'd the dilatation of the Air) that the Air, even when it is expanded to between 90 and 100 times, its extent will yet readily admit of a much further rarifaction by heat.

I consider'd also that in case the Bubbles we have been speaking of, were produc'd by the parcels of Air latitant in the Water, that Air being now got together to the top of the Tube, though the Air were again drawn out of the Receiver, the taking off its pressure would not disclose bubbles as before; and accordingly, the Air being again pump'd out, the Water in the Tube descended as formerly:

but for a great while we ſcarce ſaw one bubble appear, onely when the Receiver had been very much exhauſted, and the Water was fallen very low, there appear'd near the bottom of the Tube, certain little bubbles, which ſeem'd to conſiſt of ſuch parcels of Air as had not, by reaſon of their ſmalneſs, got up to the top of the Water, with the more bulkie and vigorous ones. And that which is not inconſiderable, is, That having, by letting in the Air, forc'd up the Water into the Tube, we could not perceive that it aſcended nearer the top, though we permitted the Engine to remain unimploy'd for two or three Nights together, and watch'd whether the Water would ſwell up and fill the Tube. And on this occaſion I remember, that having try'd ſuch an Experiment as this with Quick-ſilver inſtead of Water, in a Tube of about a Foot and a half long, wherein it might ſeem more hopeful to eſcape bubbles; yet upon the drawing down the Quick-ſilver as low as we could, and letting in the external Air upon it, we found that ſome lurking particles of Air were got up to the top of the Tube, and hinder'd the Quick-ſilver from being forc'd up again ſo high.

And

And though the Quick-ſilver were by this-means brought to appear a very cloſe and lovely Metalline Cylinder, not interrupted by interſperſ'd bubbles as before; yet having cauſ'd the Air to be again drawn out of the Receiver, I could perceive ſeveral little bubbles to diſcloſe themſelves, faſten'd to the inſide of the Tube, near the bottom of it; and having purpoſely watch'd one or two of the chiefeſt, I had the pleaſure to obſerve, that though they grew bigger and bigger as the ſurface of the Mercurial Cylinder fell nearer and nearer to them, ſo as that at length they ſwell'd into a conſpicuous bulk; yet upon the wary letting in the Air upon them, they did not break, but preſently ſhrunk up into a littleneſs that render'd them inconſpicuous.

Whence it ſeems very probable, if not certain, that even in the cloſeſt and moſt ponderous Liquors, and therefore much more in Water, there may lurk undiſcernable parcels of Air, capable, upon the removal of the preſſure of the ambient Air (though but in part) and that of the Liquor wherein it lurks, to produce conſpicuous bubbles. And conſequently, if it ſeem inconvenient to admit an Elaſtical

power in the Water, it may be ſaid that the ſwelling of the compreſſ'd Water in the Pewter Veſſel lately mention'd, and the ſpringing up of the Water at the hole made by the Needle, were not the effects of any internal *Elater* of the Water, but of the ſpring of the many little particles of Air diſperſ'd through that Water, and acting upon it in their ſudden recovering themſelves to a greater extent, then that to which a violent compreſſion had reduc'd them.

But though, from all theſe particulars, it ſeems manifeſt that the bubbles we have been all this while treating of, were produc'd by ſuch a ſubſtance as may be properly enough call'd Air; yet till we ſhall have had the opportunity of making ſome further tryals concerning the nature of the Air, we ſhall not reſolutely determine whether or no Air be a Primogenial Body (if I may ſo ſpeak) that cannot now be generated or turn'd either into Water or any other Body. Yet in the mean while (becauſe it is an important Queſtion, and if rightly determin'd, may much conduce to the knowledge of the

nature

nature of the Air) We think it not unfit to make a brief mention of some of the particulars which at present occur to our thoughts in favor of either part of the Question.

First then, divers Naturalists esteem the Air (as well as other Elements) to be ingenerable and incorruptible. And reasons plausible enough may be drawn to countenance this Opinion from the consideration of that permanency that ought to belong to the corporeal Principles of other Bodies.

Next, Experience may be pleaded to the same purpose; for I have read of some who have in vain attempted to turn Air into Water, or VVater into Air.

The diligent *Schottus* tells us, That amongst the other rarities to be met with in that great Repository of them, the *Musæum Kercherianum*, there is a round Glass with a tapering Neck near half full (as one may guess by the Scheme he annexes) of ordinary Spring-water, which having been Hermetically shut up there by *Clavius* the famous Geometrician, The included water is to this day preserv'd, not onely clear and pure, as if it were but newly put in: But (as it seems) without

Schottus Mechan hydraulico-pneumat: Part 3. Class. 1.

without (in the leaſt) turning into Air, notwithſtanding its having been kept there theſe fifty years: For he tells us, That the Water hath continued there all this while without any diminution.

Nor does it appear in thoſe Glaſſes, which for Chymical Experiments we uſually cloſe with *Hermes* his Seal (as they call it) that the included Air does, during its long Impriſonment, notwithſtanding the alteration it receives from various degrees of heat, diſcernably alter its nature. Whereas we plainly perceive in our Digeſtions and Diſtillations, that though it may be rarified into inviſible Vapors, yet it is not really chang'd into Air, but onely divided by heat, and ſcatter'd into very minute parts, which meeting together in the Alembick or in the Receiver, do preſently return into ſuch Water as they conſtituted before. And we alſo ſee, that ev'n Spirit of Wine, and other ſubtle and fugitive Spirits, though they eaſily fly into the Air, and mingle with it, do yet in the Glaſſes of Chymiſts eaſily lay aſide the diſguiſe of Air, and reſume the deveſted form of Liquors. And ſo volatile Salts, as of Urine, Harts-horn, *&c.* though they will readily diſperſe themſelves

through

through the Air, and play up and down in the capacity of an Alembick or a Receiver: yet will they, after a while, fasten themselves to the insides of such Glasses in the form of Salts.

Besides, since Air is confessedly endow'd with an Elastical power that probably proceeds from its Texture, it appears not what it is that in such light alterations of Water, as are by many presum'd capable of turning it into Air, can be reasonably suppos'd so to contrive the Particles of Water, as to give them, and that permanently, the structure requisite to a Spring. I adde the word, Permanently, because the newly mention'd observations seem to argue the Corpuscles of Air to be irreducible into Water, whereas the Aqueous Particles may perhaps for a while be so vehemently agitated, as to press almost like Springs upon other Bodies; yet upon the ceasing of the agitation, they quickly, by relapsing into Water, disclose themselves to have been nothing else whil'st they counterfeited the Air.

Lastly, The Experiment formerly made in our Engine with a piece of Match, seems to evince, that even those light and

subtle

ſubtle Fumes (for the moſt part not aqueous neither) into which the Fire it ſelf ſhatters dry Bodies, have no ſuch Spring in them as the Air, ſince they were unable to hinder or repreſs the expanſion of the Air included in the Bladder they ſurrounded.

Natural & Moral Hiſt. of the Indies, Lib. 3. C. 9.

I remember indeed that the Learned *Joſephus Acoſta*, in his Hiſtory of the *Weſt Indies*, tells us, That he ſaw in thoſe parts ſome Grates of Iron ſo ruſted and conſum'd by the Air, that the Metal being preſſ'd between the Fingers, diſſolv'd (to uſe his words) to powder, as if it had been Hay or parched Straw. And I remember too, that the accurate *Varenius* tells us, That in the Iſlands commonly called *Azores*, the Air (and Wind) is ſo ſharp, that in a ſhort time it frets not only Iron Plates, but the very Tiles upon the Roofs of Houſes, and reduces them to duſt. And I have elſewhere mention'd ſome recent Obſervations of this kinde. But it may be ſaid, That the above-mention'd Authors aſcribe the recited effects chiefly to the Winds, and that however the corroſion of the Iron and the Tiles may proceed not from the Air it ſelf, or any of its genuine parts, but from ſome

Geogr. General. Lib. 1. c. 19.

ſaline

ſaline Corpuſcles diſperſ'd through the Air, and driven by the Winds againſt the Bodies it is preſum'd to fret. And that ſuch volatile Salts may copiouſly aſcend into the Air, and yet retain their Nature, as doth the more fixt Salt in the Sea Water, the ſublimations of *Sal-Armoniack* may ſufficiently evince. Not to mention that I have ſhown ſome Friends a ſecret kinde of ſaline Subſtance incomparably ſubtler then *Sal-Armoniack*, which did not onely eaſily enough aſcend it ſelf, but carried up with it (and that in a very great proportion) the ſolid and ponderous Body ev'n of uncalcin'd Gold in the form of ſubtle exhalations, which did afterwards faſten themſelves to the upper parts of the Veſſels, and yet manifeſt themſelves to continue Gold. We remember alſo, that to try whether Water could be turn'd into Air, we once took an *Æolipile*, into which we had before convey'd ſome Water, and placing it upon kindled Coals when the heat forc'd out a vehement ſtream of aqueous Vapors; we ty'd about the neck of it, that of a Bladder, which we had before empty'd of Air; and finding the *Æolipile* after a while to blow up the Bladder, we carefully ty'd it again

that

that the included ſubſtance might not get away. Then ſlipping it off from the *Æolipile* we convey'd it into our Receiver, to try whether or no that which in part diſtended the Bladder would appear by its Spring to be true Air: whereby we found that upon the exſuction of the ambient Air, the included ſubſtance expanded it ſelf and the Bladder to a very much greater bulk then it was of before. And for further ſatisfaction, having again taken out the Bladder, we ſuffer'd it to remain ty'd up till next morning, to try whether time, and the coldneſs of the night, would make the contain'd ſubſtance relapſe into Water: But the next Morning we found it little leſs tumid then before. I remember, I ſay, that I once made this Experiment; but I might ſay in anſwer to it, that the chief reaſon of my mentioning it, is, To let Your Lordſhip ſee how requiſite it is to be circumſpect and conſiderate, when we are to make and to build upon nice Experiments. For though I may ſeem to have uſed ſufficient caution, yet afterward conſidering with my ſelf that the *Æolipile* I had imploy'd was a very large one, and that it required much more care then one that has not try'd it

would imagine, to drive out all the Air from a large *Æolipile*, I easily suspected that the distension of the Bladder in our pneumatical Vessel, might proceed not from the Watery steams that came out at the narrow mouth of the *Æolipile*, and had very much wetted the Bladder, but from the rarified Air which in that sort of Vessels is wont for a good while together to come out with the rarified Water: and accordingly having reiterated the Experiment I found it very difficult (by reason of the shrinking of the Bladders (upon their being heated) and of other impediments) to make it so accurately as to deduce from it, that Water may be rarified into true Air.

Against the four other above-mention'd Considerations, we cannot spend time to frame Objections, but must forth with proceed to the mention of those things that seem to argue that Air (at least such as produc'd our bubbles) maybe generated of Water and other Bodies.

First then we have found by Experience that a vapid Air, or Water rarified into vapor, may at least for a while emulate the elastical power of that which is generally acknowledg'd to be true Air.

For

For if you take a good *Æolipile*, with a moderately strong and slender Neck, and filling it with Water, lay it upon quick Coals, you may after a while observe so great a pressure by some of the parts contain'd in the *Æolipile* upon others, that the Water will sometimes be thrown up into the Air above three or four Foot high; and if you then take the *Æolipile* almost red hot from off the Fire, you may perceive that the Water will for a longer time then one would easily imagine continue to be spouted out in a violent Stream. And if there remains but little Water in the *Æolipile* when 'tis taken very hot from the Fire, immersing the Neck of it into cold Water, you will finde, that after it begins to suck in some Water, there will be made from time to time store of large bubbles in that Water whereinto the neck was plunged. Which bubbles seem manifestly to proceed from hence, that for a while the heat in the *Æolipile* continues strong enough to rarifie part of the Water that is suck'd in, and expel it in the form of Vapors through the Water incumbent on the Pipe. If also when the *Æolipile* is almost full of water, and therefore can contain but little

Air; you hold a Coal or Brand in that ſtream of Vapors that iſſues out of the narrow mouth of it, you will finde this vapid or rorid Air, (if I may ſo call it) to blow the Fire very ſtrongly and with a roaring noiſe. And that it be not ſaid that 'tis by the external Air which the aqueous ſteams drive before them, and not by the Steams themſelves, that the Blaſt is made and the Flame excited; it has been obſerv'd, that by approaching the Coal or Brand almoſt to the mouth of the *Æolipile*, the winde appear'd more vehement then if the Body to be kindled were held ſome Inches off.

But in regard the elaſtical power of the Stream, iſſuing out of an *Æolipile*, ſeems manifeſtly due to the heat that expands and agitates the aqueous Particles whereof that Stream conſiſts, and that ſuch rapid winds ſeem to be but water ſcatter'd into little parts and ſet a moving; ſince we finde, that holding a Knife, or any ſolid, ſmooth and cloſe Body againſt the ſtream that iſſues out of the Æolipile, the vapors condenſing upon it, will preſently cover it with water: It will be very pertinent to ſubjoyn a notable Experiment that I remember I have met with in the

description given us by the Industrious *Kircher*, of several Musical Engines. And (though it may seem somewhat prolix) we will recite what he delivers in his own words, which are these.

Kirch: Art: Mag: Con: & Disson: lib. 9. p. 309.

Cum eodem tempore quo hæc scripsi summi Pont: Innocentii X^mi *mandato organi hydraulici in horto Quirinali constituendi cura mihi commendata esset; Æoliam cameram insigni sane successu construi jussimus, eâ quæ sequitur ratione.*

See the fifteenth Figure.

Erat longitudo sive altitudo cameræ AH 5 *Pedum, Latitudine* 3 *fere ex lateribus constructa; in medio duo tenebat Diaphragmata* CD *&* EJ *in modum cribri pluribus foraminibus pertusa. Paulo infra canalis* G *aquam advehens inserebatur in* H *eidem epistomium parabat exitum. Aqua itáque per canalem* G *maximo impetu ruens vehementissimum ventum mox intus excitabat; qui ventus nimia humiditate imbutus, ut purior exiret sicciorq́ue, Diaphragmata illa in cribri modum pertusa, ordinata sunt. Intra hæc enim aquæ vehemens agitatio rupta fractaq́ue aerem puriorem per* A *canalem subtilioremque emittebat: Verum cum postea inventū sit aerē plus æquo humidū interioribus Organi meatibus maximū detrimentum inferre: Hinc ut aer aquosus*

siccissi-

ſicciſsimam cõſiſtentiam acquireret, ordina-vimus canalem plumbeum Q R *in helicem contortum vaſi* S *aliquantulum capaciori in modum Urnæ efformato, inſertum. Intra urnam enim plumbeam & canalem tortuo-ſum illiſus aer humidus, ita ab omni aquoſi-tate defæcabatur, ut ex furno in Organum derivatus dici potuerit. Urna* S *canalis tortuoſus* Q R *ultimum orificium* Q *inſe-ritur anemothecæ organi. Et hunc modum organis hydraulicis omniũ aptiſsimũ reperi.*

Debet autem camera illa ſituari in loco quantum fieri poteſt ſicciori ita ut longo ca-nali aqua intra eam derivetur ne locus hu-miditate ſua Organis officiat.

Thus far the Ingenious *Kircherus*, whom I the rather cite, becauſe although I have been informed of divers Ventiducts (as they call them) by very knowing Tra-vellers that have obſerv'd them: Yet this relation of our Author being very pun-ctual, and deliver'd upon his own particu-lar Experience, has, I confeſs, made me wiſh I had had the good fortune when I was at *Rome*, to take notice of theſe Or-gans; or that I had now the opportunity of examining of ſuch an Experiment. For if upon a ſtrict inquiry I ſhould find that the breath that blows the Organs

does not really upon the ceasing of its unusual agitation by little and little relapse into water, I should strongly suspect that 'tis possible for Water to be easily turn'd into Air. I remember indeed, that we have formerly taught that there lurks an interspersed Air in the pores of ordinary Water, which may possibly be struck out by the breaking of the Water in its fall into the Æolian Chamber, (as he calls it.) But in regard the Scheme seems to represent that Chamber as closely shut, and thereby forbids us to suppose that any Air is carried into it, but what is latitant in the Water, it will scarce seem probable to him who remembers how small a proportion of Air, that appear'd to be when its rarification seased, which was conceal'd in the Water we freed from bubbles in our Receiver, that so little Air as is commonly dispers'd through Water, should be able, in so little Water as was requisite for so small a room, to make so vehement a Wind as our Author here tells us of. I have sometime therefore suspected, that in this case the Wind may be produc'd by small particles of the water it self, forcibly expell'd out of the Chamber into the Organs. And to the Objection to which

I foreſaw this gheſs to be liable, namely, That, no heat intervening, there appear'd nothing that ſhould raiſe the Water into exhalations and give them an impulſe. I thought it might be ſaid that motion alone, if vehement enough, may, without ſenſible heat, ſuffice to break Water into very minute parts, and make them aſcend upwards, if they can no where elſe more eaſily continue their agitation. For I remember, that Travelling betwixt *Lyons* and *Geneva*, I ſaw, not very far out of the Way, a place where the River of *Rhone* coming ſuddenly to be ſtreighten'd betwixt two Rocks, ſo near each other, that a Man may (if my Memory fail me not) ſtand aſtride upon both at once: that rapid Stream daſhing with great impetuoſity againſt its Rocky Boundaries, does break part of its Water into ſuch minute Corpuſcles, and put them into ſuch a motion, that Paſſengers obſerve at a good diſtance off, as it were a Miſt ariſing from that place, and aſcending a good way up into the Air. Such, I ſay, was my ſuſpicion touching the Wind we have been conſidering, but it ſeems ſomething odde that aqueous Vapors ſhould, like a dry Wind, paſs through ſo long and tortu-

ous a Pipe of Lead, as that deſcrib'd by our Author, ſince we ſee in the Heads of Stills, and the Necks of *Æolipiles*, how quickly ſuch vapors are even by a very little cold recondenſed into Water. But to this alſo ſomething may be ſpeciouſly reply'd; wherefore contenting my ſelf to have mention'd our Authors Experiment as a plauſible, though not demonſtrative proof, that Water may be tranſmuted into Air. We will paſs on to mention in the third place another Experiment, which we try'd in order to the ſame enquiry.

We took a clear Glaſs bubble (capable of containing by gheſs about three Ounces of Water) with a Neck ſomewhat long and wide, of a Cylindrical form; this we fill'd with Oyl of Vitriol and fair water, of each almoſt a like quantity, and caſting in half a dozen ſmall Iron Nails, we ſtopt the mouth of the Glaſs (which was top-full of Liquor) with a flat piece of *Diapalma* provided for the purpoſe, that accommodating it ſelf to the ſurface of the water, the Air might be exquiſitely excluded: and ſpeedily inverting the Viol, we put the Neck of it into a ſmall wide-mouth'd Glaſs that ſtood ready with more of the ſame Liquor in it, to

re-

receive it. As ſoon as the neck had reach'd the bottom of the Liquor it was dipp'd into, there appear'd at the upper part (which was before the bottom) of the Viol a bubble, of about the bigneſs of a Pea, which ſeem'd rather to conſiſt of ſmall and recent bubbles, produc'd by the action of the diſſolving Liquor upon the Iron, then any parcel of the external Air that might be ſuſpected to have got in upon the inverſion of the Glaſs, eſpecially ſince we gave time to thoſe little Particles of Air which were carried down with the Nails into the Liquor to fly up again. But whence this firſt bubble was produced, is not ſo material to our Experiment, in regard it was ſo ſmall: For ſoon after we perceiv'd the bubbles produced by the action of the *Menſtruum*, upon the Metal aſcending copiouſly to the bubble already named, and breaking into it, did ſoon exceedingly increaſe it, and by degrees depreſs the water lower and lower, till at length the ſubſtance contain'd in theſe bubbles poſſeſſed the whole cavity of the Glaſs Viol, and almoſt of its Neck too, reaching much lower in the Neck then the ſurface of the ambient Liquor, wherewith the open-mouth'd Glaſs was by this means almoſt

replenished. And because it might be suspected that the depression of the Liquor might proceed from the agitation whereinto the exhaling and imprison'd steams were put, by that heat which is wont to result from that action of corrosive salts upon Metals, we suffered both the Viol and the open-mouthed Glass to remain as they were, in a Window, for three or four days and nights together; but looking upou them several times during that while, as well as at the expiration of it, the whole cavity of the Glass bubble, and most of its Neck, seem'd to be possess'd by Air, since by its spring it was able for so long to hinder the expell'd and ambient Liquor from regaining its former place. And it was remarkable, that just before we took the Glass bubble out of the other Glass, upon the application of a warm hand to the convex part of the bubble; the Imprison'd substance readily dilated it self like Air, and broke through the Liquor in divers bubbles, succeeding one another.

Having also another time try'd the like Experiment with a small Viol, and with Nails dissolv'd in *Aquafortis*, we found nothing incongruous to what we have now deliver'd. And this Circumstance

we

we obſerv'd, that the newly generated ſteams did not onely poſſeſs almoſt all the whole cavity of the Glaſs, but divers times without the aſſiſtance of the heat of my hand, broke away in large bubbles through the ambient Liquor into the open Air: So that theſe Experiments with corroſive Liquors, ſeem'd manifeſtly enough to prove, though not that Air may be generated out of the Water, yet that in general air may be generated anew.

Laſtly, to the foregoing Arguments from Experience we might eaſily ſubjoyn the Authority of *Ariſtotle*, and of (his followers) the Schools who are known to have taught, that Air and Water being Symbolizing Elements (in the quality of moiſture) are eaſily tranſmutable into one another. But we ſhall rather to the foregoing Argument adde this, drawn from Reaſon, That if, as *Leucippus*, *Democritus*, *Epicurus* and others, follow'd by divers modern Naturaliſts, have taught, the difference of Bodies proceeds but from the various Magnitudes, Figures, Motions, and Textures of the ſmall parts they conſiſt of, (all the qualities that make them differ, being deducible from thence) there appeares

no reason why the minute parts of Water, and other Bodies, may not be so agitated or connected as to deserve the name of Air. For if we allow the *Cartesian Hypothesis*, according to which, as we noted at the beginning of this Letter, the Air may consist of any terrene or aqueous Corpuscles, provided they be kept swimming in the interfluent Celestial Matter; it is obvious that Air may be as often generated, as Terrestrial Particles minute enough to be carried up and down, by the Celestial Matter ascend into the Atmosphere. And if we will have the Air to be a *congeries* of little slender Springs, it seems not impossible, though it be difficult, that the small parts of divers Bodies may by a lucky concourse of causes be so connected as to constitute such little Springs, since (as we note in another Treatise) Water in the Plants it nourishes is usually contriv'd into Springy Bodies, and even the bare alter'd position and connexion of the parts of a Body may suffice to give it a Spring that it had not before, as may be seen in a thin and flexible Plate of Silver; unto which, by some stroaks of a Hammer, you may give a Spring, and by onely heating it red hot you

you may make it again flexible as before.

These, My Lord, are some of the Considerations at present occurring to my thoughts, by which it may be made probable that Air may be generated anew. And though it be not impossible to propose Objections against these, as well as against what has been represented in favor of the contrary Doctrine; yet having already almost tyr'd my self, and I fear more then almost tyr'd Your Lordship with so troublesome an Enquiry after the Nature of bubbles, I shall willingly leave Your Lordship to judge of the Arguments alledged on either side, and I should scarce have ventur'd to entertain You so long concerning such empty things as the Bubbles, which have occasion'd all this Discourse, but that I am willing to invite You to take notice with me of the obscurity of things, or the dimness of our created Intellects (which yet of late too many so far presume upon, as either to Deny or Censure the Almighty and Omniscient Creator himself) and to learn hence this Lesson, That there are very many Things in Nature that we disdainfully over-look as obvious or despicable,

cable, each of which would exercise our Understandings, if not pose them too, if we would but attentively enough consider it, and not superficially contemplate, but attempt satisfactorily to explicate the nature of it.

Experiment 23. SInce the writing of the twenty one and twenty second Experiments (and notwithstanding all that hath been on their occasion deliver'd concerning bubbles) we made some further tryals in prosecution of the same inquiry whereto they were designed.

We chose then, amongst those Glasses which Chymists are wont to call Philosophical Eggs, one that containing about nine Ounces of Water, had a Neck of half an Inch in Diameter at the top, and as we ghest, almost an Inch at the bottom; which breadth we pitch'd upon for a reason that will by and by appear: then filling it with common Water to the height of about a Foot and a half, so that the upper part remain'd empty, we shut it into the Receiver, and watch'd what would follow upon pumping, which proved

ved that a great part of the Air being drawn out, the bubbles began to diſcover themſelves at the bottom and ſides of the Glaſs; and increaſing, as the Air was more and more drawn away, they did from time to time aſcend copiouſly e-nough to the top of the Water, and there quickly break: but by reaſon that the wideneſs of the Glaſs allow'd them free paſſage through the Water, they did not appear as in the former Experiments to make it ſwell: The Water ſcarce ever ri-ſing at all above the mark affixt to its up-per ſurface when it was put in, and upon the return permitted to the outward Air, and conſequently the ſhrinking in of the remaining bubbles, the Water ſeem'd to have loſt of its firſt extent, by the avo-lation of the formerly interſperſ'd Air.

Being willing likewiſe to try whether diſtilled Water were by having been di-vided into minute parts, and then re-uni-ted, more or leſs diſpoſ'd to expand it ſelf then Water not diſtill'd: We took out of our Laboratory ſome careful-ly diſtill'd Rain-water, and put about two Ounces of it into a round Glaſs

bubble,

bubble with a very ſmall Neck (not exceeding the ſixth part of an Inch in Diameter) which we fill'd half way to the top, and then convey'd it into the Receiver; the iſſue was, That though we drew out more then ordinary, yet there appear'd not the leaſt intumeſcence of the Water, nor any aſcending bubbles.

But ſuſpecting that either the ſmall quantity of the water or the Figure of the Veſſel might have an intereſt in this odde *Phænomenon*, we took the lately mention'd Philoſophical Egge, and another not much differing from it; the former we fill'd up with diſtill'd Rain-water to the old mark, and into the latter we put a long Cylinder or Rod of ſolid Glaſs to ſtreighten the cavity of the Neck by almoſt filling it up; and then pouring ſome diſtilled Water into that alſo, till it reach'd within ſome Fingers breadth of the top, the Eggs were let down into the Receiver. In this Experiment the Air was ſo far drawn forth before there appear'd any bubble in either of the Glaſſes, that the diſparity betwixt this and common water was manifeſt enough. But at length, when the Air was almoſt quite pump'd out, the bubbles began to diſ-

cloſe

close themselves, and to increase as the pressure of the Air in the Receiver decreas'd. But whereas in the first mention'd Philosophical Egge the bubbles were very small, and never able to swell the Water, that we took notice of, at all above the mark: In the other, whose Neck, as we lately said, was straightned, and their passage obstructed, great numbers of them, and bigger, fastned themselves to the lower end of the Glass rammer (if we may so call it) and gather'd in such numbers between that and the sides of the Neck, that the Water swell'd about a Fingers breadth above the mark, though upon the admitting of the external Air it relaps'd to the former mark, or rather fell somewhat below it. And although thereupon in the first nam'd Vessel all the bubbles presently dis-appear'd, yet in the other we observ'd, that divers remained fastned to the lower part of the Glass rammer, and continued there somewhat to our wonder, for above an hour after, but contracted in their Dimensions.

Moreover, having suffered the Glasses to remain above twenty four hours in the

Receiver,

Receiver, we afterwards repeated the Experiment, to try what change the ex ſuction of the external Air would produce in the Water, after the internal and latitant Air had (as is above recited) in great meaſure got away in bubbles, and whether or no the Water would by ſtanding re-admit any new particles of Air in the room of thoſe that had forſaken it. But though we exhauſted the Receiver very diligently, yet we ſcarce ſaw a bubble in either of the Glaſſes; notwithſtanding which, we perceiv'd the Water to riſe about the breadth of a Barly-corn, or more, in the Neck of that Glaſs wherein the ſolid Cylinder had been put; The Liquor in the other Glaſs not ſenſibly ſwelling.

And laſtly, upon the letting in of the Air, the Water in the ſtraightned Neck ſoon ſubſided to the mark above which it had ſwollen, which whether it ought to be aſcrib'd to the ſame ſmall expanſion of the parts of the Water it ſelf, or to the rarifaction of ſome yet latitant Air broken into ſuch ſmall particles, as to eſcape our obſervation, ſeems not eaſily determinable, without ſuch further tryals, as would perhaps prove tedious to be recited as well as to be made; though I was

con-

consent to set down those already mention'd, that it might appear how requisite it is in nice Experiments to consider variety of Circumstances.

After having thus discover'd what operation the exsuction of the ambient Air had upon Water, we thought good to try also what changes would happen in other Liquors upon the like taking off the pressure of the external Air. We took then a Glass Egge, somewhat bigger then a Turkey Egge, which had a long Neck or Stem of about a $\frac{1}{3}$ part of an Inch in Diameter; and filling it up with Sallet Oyl until it reach'd above half way to the top of the Neck, we inclos'd it in the Receiver together with common Water in a resembling Vessel, that we might the better compare together the operation of the exsuction of the Air upon those two Liquors. The Pump being set awork there began to appear bubbles in the Oyl much sooner then in the Water, and afterwards they also ascended much more copiously in the former Liquor then the latter: Nay, and when by having quite tired the Pumper, and almost our own patience, we *Experiment 24.*

gave over, the bubbles rise almost (if not altogether) in as great numbers as ever, insomuch as none of the various Liquors we tryed either before or since, seem'd to abound more with Aerial Particles then did this Oyl. In which it was further remarkable, that between the time it was set into the Receiver, and that at which we could get ready to Pump, it subsided notably (by ghess about half an Inch) below the mark it reach'd before it was put in.

After this express'd Oyl, we made tryal of a distill'd one, and for that purpose made choice of the common Oyl or Spirit (for in the Shops where it is sold, the same Liquor is promiscuously call'd by either name) of Turpentine; because 'twas onely of that Chymical Oyl, we had a sufficient quantity: which, being put into a small Glass bubble with a slender Neck, so as to fill it to about two Inches from the top, did, upon the evacuating of the Receiver, present us with great store of bubbles; most of which rising from the bottom, expanded themselves exceedingly in their ascent, and made the Liquor in the Neck to swell so much by degrees, that at length it divers times ran over

over at the top: by which means, we were hindred from being able to discern upon the letting in of the Air, how much the subsidence of the Oyl below the first mark was due to the recess of the bubbles.

Having likewise a minde to try whether as strong a solution of Salt of Tartar in fair Water as could be made (we having then no Oyl of Tartar *per deliquium* at hand) though it be accounted, Quick-silver excepted, the heaviest of Liquors would afford us any bubbles; we put in a Glass Egge full of it at the same time, with other Liquors, and found that they did long yield store of bubbles before any discovered themselves in the Liquor of Tartar; and having pursued the Experiment, it appear'd, That of all the Liquors we made tryal of, this afforded the fewest and the smallest Bubbles.

Spirit of Vinager being try'd after the same manner, exhibited a moderate number of bubbles, but scarce any thing else worth the mentioning.

Nor could we in red Wine, try'd in a Glass Egge, take notice of any thing very observable. For though upon the exsuction of the Air the bubbles ascended

in this Liquor, as it were in sholes, and shifted places among themselves in their ascent; yet the Intumescence of the whole bulk of the Liquor was scarce at all sensible, the bubbles most commonly breaking very soon after their arrival at the top, where during their stay, they compos'd a kinde of shallow froth, which alone appear'd higher in the Neck of the Glass, then was the Wine when it was first let down. Neither yet did Milk, convey'd into our Pneumatical Vessel, present us with any thing memorable, save that (as it seem'd by reason of some unctuousness of the Liquor) the bubbles not easily breaking at the top, and thrusting up one another made the intumescence appear much greater then that of common Water.

We likewise convey'd Hens Eggs into the Receiver, but, after the exsuction of the Air, took them out whole again. That which invited us to put them in, was, That (as perhaps we mention in other Papers) we had among other Experiments of cold, made Eggs burst, by freezing them within doors with Snow and Salt: The Ice, into which the aqueous parts of the Egge were turned by the cold, so distending

ing (probably by reason of the numerous bubbles wont to be observable in Ice) the outward parts of the Egge, that it usually crack'd the shell, though the inner Membrane that involv'd the several Liquors of the Egge, because it would stretch and yield, remain'd unbroken. And hereupon we imagin'd that in our Engine it might appear whether or no there were any considerable Spring, either in any of the Liquors, or in any other more spirituous substance included in the Egge.

We took also some Spirit of Urine, carelesly enough deflegmed, and put it into the same Glass (first carefully scowr'd and cleans'd) wherein we had put the Oyl-olive above mention'd: We took also another Glass, differing from a Glass Egge, onely in that its bottom was flat, and fill'd it up to about $\frac{2}{3}$ of the Neck (which was wider then that of the Egge) with rectified Spirit of Wine.

We took also another Glass Egge, and having fill'd it with common Water till it reach'd to the middle of the Neck, we pour'd to it of the same Spirit of Wine, till it reach'd about an Inch higher.

These three Glasses having marks set on them, over against the edges of the

contain'd Liquors were put into the Receiver, and that beginning to be evacuated, the bubbles in all the three Liquors began to appear. The mixture of the Spirit of Wine and Water disclos'd a great store of bubbles, especially towards the top; but scarce afforded us any thing worth remembring. The Spirit of Urine appear'd to swell near an Inch and an half above the mark; and besides that, sent forth store of bubbles, which made a kinde of froth at the upper part of it. And above that spume there appear'd eight or ten great bubbles one above another, in a very decent order, each of them constituting, as it were, a Cylinder of about half an Inch high, and as broad as the internal cavity of the Neck: So that all the upper part of the Neck (for these bubbles reach'd to the top) seem'd to be divided into almost equal parts, by certain Diaphragmes, consisting of the coats of the bubbles, whose edges appear'd like so many Rings suspended one above another.

In the Spirit of Wine there did arise a great multitude of bubbles, even till weariness did make us give over the Experiment. And in these bubbles two or three things were remarkable; as first,

That

That they aſcended with a very notable celerity: Next, That being arriv'd at the top, they made no ſtay there, and yet, notwithſtanding the great thinneſs and ſpirituouſneſs of the Liquor, did, before they broke, lift up the upper ſurface of it, and for a moment or two form thereof a thin film or skin which appear'd protuberant above the reſt of the ſuperficies like a ſmall Hemiſphere. Thirdly, That they aſcended ſtraight up, whereas thoſe produc'd at the lower part of the Veſſel, containing the mixture of the Water and Spirit of Wine, aſcended with a wavering or wrigling motion, whereby they deſcrib'd an indented Line. Laſtly, It was obſervable in the Spirit of Wine (and we took notice of the like in the Oyl of Turpentine lately mention'd) that not onely the bubbles ſeem'd to riſe from certain determinate places at the bottom of the Glaſs, but that in their aſcenſion they kept an almoſt equal diſtance from each other, and follow'd one another in a certain order, whereby they ſeem'd part of ſmall Bracelets, conſiſting of equally little incontiguous Beads: the lower end of each Bracelet, being as it were, faſten'd to a certain point at the bottom of the Glaſs.

The Air being ſparingly let into the Receiver, the great bubbles formerly mention'd as incumbent upon one another, in that Glaſs that contain'd the Spirit of Urine, were by orderly degrees leſſen'd, till at length they wholly ſubſided, notwithſtanding the receſs of ſo many bubbles as broke on the top of the Spirit of Urine, during all the time of the Experiment; yet it ſcarcely appear'd at all to be ſunk below the mark: Nor did the mixture of Spirit of Wine and Water conſiderably ſubſide. But that is nothing to what we obſerv'd in the Spirit of Wine, for not onely it conſpicuouſly expanded it ſelf in the Neck of the Veſſel that contain'd it, notwithſtanding the largeneſs of it; and that the bubbles were about to break at the top of it almoſt aſſoon as they arriv'd there: but upon the re-admiſſion of the external Air, the Spirit of Wine retain'd its newly acquired expanſion. And though we let it alone for near an hour together, in expectation that it might ſubſide; yet when we took it out, we found it ſtill ſwell'd between a quarter and half an Inch above the mark; and although it was not eaſily imagi-

imaginable how this *Phænomenon* could proceed from any miſtake in trying the Experiment, yet the ſtrangeneſſe of it invited me to repeat it with freſh Spirit of Wine; which, ſwelling in the Neck as formerly, I left all Night in the Receiver, allowing free acceſs to the external Air at the Stop-cock, and the next day found it ſtill expanded as before, ſave that it ſeem'd a little lower: which decrement perhaps proceeded from the avolation of ſome of the fugitive parts of ſo volatile a Liquor. And for better ſatisfaction having taken out the Glaſs, and conſider'd it in the open Air, and at a Window, I could not finde that there was any remaining Bubbles that could occaſion the perſevering and admir'd expanſion.

Experiment 25.

BEing deſirous to diſcover what difference there might be as to gravity and levity, between Air expanded under Water, and it ſelfe before ſuch expanſion; we took two very ſmall Viols, ſuch as Chymical Eſſences (as they call them) are wont to be kept in, and of the ſize and ſhape expreſſed by the 8th Figure: into one of theſe

we put so much of a certain ponderous Mercurial mixture (hapning to be then at hand) that the mouth being stopt with a little soft Wax, the Glass would just sink in Water and no more; this we let fall to the bottom of a wide-mouth'd Crystal Jar, fill'd with about half a pint of common Water, and into the same Vessel we sunk the other Essence Glass unstopp'd, with as much Water in it as was more then sufficient to make it subside. Both these sunk with their mouthes downward, the former being about three quarters full of Air, the latter containing in it a bubble of Air that was ghess'd to be of the bigness of half a Pea: This done, the wide-mouth'd Glass was let down into the Receiver, and the way of imploying the Engine was carefully made use of.

The success was, That having drawn out a pretty quantity of Air, the bubbles began to disclose themselves in the Water, as in the former Experiments; and though for a good while after the bubbles ascended in swarms from the lower parts of the Water, and hastily broke at the top; yet we prosecuted the Experiment so long without seeing any effect wrought

upon

upon the Essence Bottles, that we began to dispair of seeing either of them rise, but continuing to ply the Pump, that little Glass, whose mouth was open'd, came to the top of the Water, being, as it were, boy'd up thither by a great number of bubbles that had fastned themselves to the sides of it; swimming thus with the mouth downward, we could easily perceive that the internal Air above mention'd had much delated it self, and thereby seem'd to have contributed to the emerging of the Glass, which remain'd floating, notwithstanding the breaking and vanishing of most of the contiguous bubbles: being hereby incouraged to persist in pumping, we observed with some pleasure, that at each time we turn'd the Key, the Air in the little Glass did manifestly expand it self and thrust out the water, generally retaining a very protuberant surface where it was contiguous to the remaining Water. And when after divers exsuctions of the Air in the Receiver, that in the little Viol so dilated it self as to expel almost all the Water, it turn'd up its mouth towards the surface of the Water in the Jar, and there deliver'd a large bubble, and then relapsed into its

for-

former floating posture: And this Experiment taught us, among other things, that it was a work of more time and labor then we imagin'd, to exhaust our Engine as much as it may be exhausted: for although before the emerging of the small Viol, we did (as has been touch'd already) think we had very considerably emptyed the Receiver, because there seem'd to come out but very little or almost no sensible Air at each exsuction into and out of the Cylinder; yet afterwards, at each drawing down the Sucker, the Air included in the Viol did manifestly dilate it self, so long, that it did no less then nine times turn its mouth upwards, and discharge a bubble by conjecture about the bigness of a Pea, after the manner newly recited. But as for that Violl which had the weight in it, it rose not at all. So that being not able by quick pumping to gain another bubble from the Air in the swimming Glass, which proceeded from some small leak in the Vessel, though it held in this Experiment more stanch then was usual, we thought fit to let in leasurely the Air from without, upon whose admission that with-

in the Viol ſhrinking into a very narrow compaſs, the Glaſs did, as we expected, fall down to the bottom of the Jar.

But being deſirous before we proceeded to any new Experiment, to try once more whether the little Glaſs that had the weight in it might not alſo be raiſ'd. After we had ſuffer'd the Engine to remain cloſ'd as it was, for five or ſix hours, the Pump was again ply'd with ſo much obſtinacy, that not onely about the upper part of the Jar there appear'd a good number of bubbles (but very much ſmaller then thoſe we ſaw the firſt time) but afterwards there came from the bottom of the Jar, bubbles about the bigneſs of ſmal Peas: which the Pump being ſtill kept going, follow'd one another, to the number of forty, coming from the ſtopp'd Violl; whoſe mouth, it ſeems, had not been ſhut ſo ſtrongly and cloſely, but that the included Air, dilating it ſelf by its own ſpring, made it ſelf ſome little paſſage betwixt the Wall and the Glaſs, and got away in theſe bubbles; after which, the unſtopp'd Glaſs began to float again, the Air ſhut up in it being

being manifestly so dilated as to expel a good part of the Water, but not so much as to break quite thorow. And at length, when our expectation of it was almost tired out, the heavier of the two Viols began to come aloft, and immediately to subside again, which appear'd to be occasion'd by the Air within it, whose bulk and spring being weaken'd by the recess of the forty bubbles before-mention'd, it was no longer able, as formerly, to break forcibly through the incumbent Water; but forming a bubble at the mouth of the Glass, boyed it up towards the top, and there getting away, left it to sink again till the pressure of the Air in the Receiver being further taken off, the Air in the Viol was permitted to expand it self further, and to create another bubble, by which it was again for a while carried up. And it was remarkable, that though after having emptyed the Receiver as far as well we could, we ceas'd from pumping; yet the Vessel continuing more stanch then it was wont, this ascent and fall of the Viol was repeated to the ninth time; the included Air, by reason of the smalness of the vent at which it must pass out, being not able to get away otherwise then

little

little by little; and consequently, in divers such parcels as were able to constitute bubbles, each of them big enough to raise the Viol and keep it aloft until the avolation of that bubble. Whereby it may appear, that the grand rule in *Hydrostaticks*, That a Body will swim in the Water, in case it be lighter then as much of that Water that equals it in bulk, will hold likewise when the pressure of the Atmosphere is in very great measure, if not when it is totally taken off from the Liquor and the Body: though it were worth inquiring what it is that so plentifully concurs to fill the bubbles made in our Experiment by the so much expanded Air, for to say with the old Peripatetick Schools, That the Air, in Rarefaction, may acquire a new extent, without the admission of any new substance, would be an account of the *Phænomenon* very much out of date, and which, I suppose, our Modern Naturalists would neither give, nor acquiess in.

I know not whether it may be requisite to adde, that in this Experiment, as in the former, the outward Air being let in did soon precipitate the floating Viol. But I think it will not be amiss to note, that

(con-

(congruously to what hath been above recorded of the vast expansion of the Air) the Water which in the heavier Viol succeeded in the room of those forty odde, if not fifty great bubbles of Air, which at several times got out of it, amounted but to a very inconsiderable bigness.

Experiment 26. IT having been observ'd by those that have consider'd what belongs to *Pendulums* (a Speculation that may, in my poor judgement, be highly useful to the Naturalists) that their Vibrations are more slowly made, and that their motion lasts less in a thicker, then in a thinner Medium: We thought it not amiss to try if a *Pendulum* would swing faster, or continue swinging longer in our Receiver, in case of the exsuction of the Air, then otherwise. Wherefore we took a couple of round and polish'd *Pendulums* of Iron or Steel, of equal bigness, as near as we could get the Artificer to make them, and weighing each of them twenty Dragmes, wanting as many Grains. One of these we suspended in the cavity of the Receiver by a very slender silken string, of about seven Inches and a half in length from

from the cover of the Receiver to which it was fasten'd. Then (by inclining the Engine) we made the *Pendulum* swing too and fro in it, and describ'd as long Arches as in the capacity of so brittle a Vessel we thought safe and convenient. And one of the Assistants telling the recursions of the other *Pendulum* hanging in the free Air, by a string of about the same length, we shorten'd and lengthen'd this other *Pendulum*, till it appear'd to keep the same pace in its Vibrations, with that shut up in the Receiver. Then having carefully drawn away the Air, we did again set the *Pendulum* in the Receiver a vibrating; and giving the other *Pendulum* such a motion as made it describe an Arch, according to ones ghess, equal to that of the included *Pendulum*; we reckon'd, one of us, the Recursions of that *Pendulum* which was swinging within the Receiver; and another of us that which was moving in (that which one would think a much more resisting *medium*) the Air. But once, one of us reckon'd near two and twenty Recursions of the included *Pendulum*, whilst the other reckon'd but twenty of the *Pendulum* that vibrated without. And another time also, the former of these *Pen-*

dula was reckon'd to have made one and twenty Recursions, wherein the other made but twenty: Yet this Experiment seem'd to teach us little, save that the difference betwixt the motion of such a *Pendulum* in the common Air, and in one exceedingly rarified, is scarce sensible in Vessels no bigger then our Receiver; especially since though during this Experiment it held very well, yet we could not suppose it to be altogether devoid of Air. We observ'd also, that when the Receiver was full of Air, the included *Pendulum* continu'd its Recursions about fifteen minutes (or a quarter of an hour) before it left off swinging; and that after the exsuction of the Air, the Vibration of the same *Pendulum* (being fresh put into motion) appear'd not (by a minutes Watch) to last sensibly longer. So that the event of this Experiment being other then we expected, scarce afforded us any other satisfaction, then that of our not having omitted to try it. And whether in case the tryal be made with a *Pendulum* much less disproportionate to the Air then Steel is, the event will much better answer expectation, experience may be consulted.

That

THat the Air is the medium whereby ſounds are convey'd to the Ear, has been for many Ages, and is yet the common Doctrine of the Schools. But this Received Opinion has been of late oppoſ'd by ſome Philoſophers upon the account of an Experiment made by the Induſtrious *Kircher*, and other Learned Men, who have (as they aſſure us) obſerv'd, That if a Bell, with a Steel Clapper, be ſo faſten'd to the inſide of a Tube, that upon the making the Experiment *De Vacuo* with that Tube, the Bell remain'd ſuſpended in the deſerted ſpace at the upper end of the Tube: And if alſo a vigorous Load-ſtone be apply'd on the outſide of the Tube to the Bell, it will attract the Clapper, which upon the Removal of the Load-ſtone falling back, will ſtrike againſt the oppoſite ſide of the Bell, and thereby produce a very audible ſound; whence divers have concluded, That 'tis not the Air, but ſome more ſubtle Body that is the medium of ſounds. But becauſe we conceiv'd that, to invalidate ſuch a conſequence from this ingenious Experiment (though the moſt lucife- *Experiment 27.*

rous, that could well be made without ſome ſuch Engine as ours) ſome things might be ſpeciouſly enough alleadg'd; we thought fit to make a tryal or two, in order to the Diſcovery of what the Air does in conveying of ſounds, reſerving divers other Experiments tryable in our Engine concerning ſounds, till we can obtain more leaſure to proſecute them. Conceiving it then the beſt way to make our tryal with ſuch a noiſe as might not be loud enough to make it difficult to diſcern ſlighter variations in it, but rather might be, both laſting, that we might take notice by what degrees it decreaſ'd; and ſo ſmall, that it could not grow much weaker without becoming imperceptible. We took a Watch, whoſe Caſe we open'd, that the contain'd Air might have free egreſs into that of the Receiver. And this Watch was ſuſpended in the cavity of the Veſſel onely by a Pack-thred, as the unlikelieſt thing to convey a ſound to the top of the Receiver: And then cloſing up the Veſſel with melted Plaiſter, we liſten'd near the ſides of it, and plainly enough heard the noiſe made by the ballance. Thoſe alſo of us, that watch'd for that Circumſtance, obſerv'd, that the noiſe ſeem'd to come

come directly in a straight Line from the Watch unto the Ear. And it was observable to this purpose, that we found a manifest disparity of noise, by holding our Ears near the sides of the Receiver, and near the Cover of it: which difference seem'd to proceed from that of the Texture of the Glass, from the structure of the cover (and of the Cement) through which the sound was propagated from the Watch to the Ear. But let us prosecute our Experiment. The Pump after this being imployd, it seemd that from time to time the sound grew fainter and fainter; so that when the Receiver was empty'd as much as it us'd to be for the foregoing Experiments, neither we, nor some strangers that chanc'd to be then in the room, could, by applying our Ears to the very sides, hear any noise from within; though we could easily perceive that by the moving of the hand which mark'd the second minutes, and by that of the ballance, that the Watch neither stood stil, nor remarkably varied from its wonted motion. And to satisfie our selvs further that it was indeed the absence of the Air about the Watch that hinder'd us from hearing it, we let in the external Air at the Stop-cock, and then though we

turn'd the Key and ſtopt the Valve, yet we could plainly hear the noiſe made by the ballance, though we held our Ears ſometimes at two Foot diſtance from the outſide of the Receiver. And this Experiment being reiterated in another place, ſucceded after the like manner. Which ſeems to prove, that whether or no the Air be the onely, it is at leaſt, the principal medium of Sounds. And by the way it is very well worth noting, that in a Veſſel ſo well cloſ'd as our Receiver, ſo weak a pulſe as that of the ballance of a Watch ſhould propagate a motion to the Ear in a Phiſically ſtraight Line, notwithſtanding the interpoſition of ſo cloſe a Body as Glaſs, eſpecially Glaſs of ſuch thickneſs as that of our Receiver; ſince by this it ſeems that the air impriſon'd in the Glaſs, muſt, by the motion of the ballance, be made to beat againſt the concave part of the Receiver, ſtrongly enough to make its convex part beat upon the contiguous Air, and ſo propagate the motion to the Liſtners ears. I know this cannot but ſeem ſtrange to thoſe, who, with an eminent Modern Philoſopher, will not allow that a Sound, made in the cavity of a Room, or other place ſo cloſ'd, that there

is no intercourſe betwixt the external and internal Air, can be heard by thoſe without, unleſs the ſounding Body do immediately ſtrike againſt ſome part of the incloſing Body. But not having now time to handle Controverſies, we ſhall onely annex, That after the foregoing Experiment, we took a Bell of about two Inches in Diameter at the bottom, which was ſupported in the midſt of the cavity of the Receiver by a bent ſtick, which by reaſon of its Spring preſſ'd with its two ends againſt the oppoſite parts of the inſide of the Veſſel: in which, when it was cloſ'd up, we obſerv'd that the Bell ſeem'd to ſound more dead then it did when juſt before it ſounded in the open Air. And yet, when afterwards we had as formerly emptyed the Receiver, we could not diſcern any conſiderable change (for ſome ſaid they obſerv'd a ſmall one) in the loudneſs of the ſound, whereby it ſeem'd that though the Air be the principal medium of ſound, yet either a more ſubtle matter may be alſo a medium of it, or elſe an ambient Body that contains but very [illegible] particles of Air, in compariſon of thoſe it is eaſily capable of, is ſufficient for that purpoſe. And this, among o-

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ther things, invited us to consider, whether in the above-mention'd Experiment made with the Bell and the Load-stone, there might not in the deserted part of the Tube remain Air enough to produce a sound: since the Tubes for the Experiment *De Vacuo* (not to mention the usual thinness of the Glass) being seldom made greater then is requisite, a little Air might bear a not inconsiderable proportion to the deserted space. And that also, in the Experiment *De Vacuo*, as it is wont to be made, there is generally some little Air that gets in from without, or at least store of bubbles that arise from the Body of the Quick-silver, or other Liquor it self, Observations heedfully made have frequently informed us: And it may also appear, by what has been formerly delivered concerning the *Torricellian* Experiment.

On the occasion of this Experiment concerning sounds, we may adde in this place, That when we try'd the Experiment formerly mention'd, of firing Gun-powder with a Pistol in our evacuated Receiver, the noise made by the striking of the Flint against the Steel, was exceeding languid in comparison of what it would

have

have been in the open Air. And on divers other occasions it appear'd that the sounds created within our exhausted Glass, if they were not lost before they reach'd the Ear, seem'd at least to arrive there very much weaken'd. We intended to try whether or no the Wire-string of an Instrument shut up into our Receiver, would, when the ambient Air was suck'd out, at all tremble, if in another Instrument held close to it, but without the Receiver a string tun'd (as Musicians speak, how properly I now examine not) to an Unison with it, were briskly toucht, and set a Vibrating. This, I say, we purpos'd to try to see how the motion made in the Air without, would be propagated through the cavity of our evacuated Receiver. But when the Instrument wherewith the tryal was to be made came to be imploy'd, it prov'd too big to go into the Pneumatical Vessel, and we have not now the conveniency to have a fitter made.

We thought likewise to convey into the Receiver a long and slender pair of Bellows, made after the fashion of those usually employ'd to blow Organs, and furnish'd with a small Musical instead of an ordi-

ordinary Pipe. For we hop'd, that by means of a ſtring faſtned to the upper part of the Bellows, and to the moveable ſtopple that makes a part of the Cover of our Receiver, we ſhould, by frequently turning round that ſtopple, and the annexed ſtring, after the manner already often recited, be able to liſt up and diſtend the Bellows; and by the help of a competent weight faſten'd to the ſame upper part of the Bellows, we ſhould likewiſe be able, at pleaſure, to compreſs them: and by conſequence, try whether that ſubtler matter then Air (which, according to thoſe that deny a *Vacuum*, muſt be ſuppoſ'd to fill the exhauſted Receiver) would be able to produce a ſound in the Muſical Pipe; or in a Pipe like that of ordinary Bellows, to beget a Wind capable to turn or ſet a moving ſome very light matter, either ſhap'd like the Sails of a Wind-Mill, or of ſome other convenient form, and expoſ'd to its Orifice. This Experiment, I ſay, we thought to make, but have not yet actually made it for want of an Artificer to make us ſuch a pair of Bellows as it requires.

We had thoughts alſo of trying whether or no, as Sounds made by Bodies in

our Receiver become much more languid then ordinary, by reason of the want of Air, ſo they would grow ſtronger, in caſe there were an unuſual quantity of Air crouded and ſhut up in the ſame Veſſel, which may be done (though not without ſome difficulty) by the help of the Pump, provided the Cover and Stopple be ſo firmly faſten'd (by binding and Cement, or otherwiſe) to the Glaſs; and to each other, that there be no danger of the condenſ'd Airs blowing of either of them away, or its breaking through the junctures. Theſe thoughts, My Lord, as I was ſaying, we entertain'd; but for want of leaſure, as, of as good Receivers as ours, to ſubſtitute in its place, in caſe we ſhould break it before we learn'd the skill of condencing the Air in it, we durſt not put them in practice: Yet, on this occaſion, give me leave to advertiſe Your Lordſhip once for all, That though for the reaſons newly intimated, we have, Onely in the ſeventeenth Experiment, taken notice, that by the help of our Engine the Air may be condenſ'd as well as rarified; yet there are divers other of our Experiments, whoſe *Phænomena* it were worth

worth while to try to vary, by means of the compression of the Air.

Experiment 28. WE taught, among divers other things, when we discours'd of our first Experiment, That the Air shut up in our Receiver, presseth as strongly upon the Bodies shut up with it, as if they were expos'd to the pressure of the whole Atmosphere. That this was not inconsiderately propounded, we hope Your Lordship has gather'd from divers of the things already recited: But yet perhaps it will not be amiss to subjoyn, by way of further confirmation of the same truth, the following Experiment, which should have accompanied the 20th, but the Paper where in the one was written chanc'd not to be at hand, when the other was sent away.

We convey'd into the Receiver a new Glass Viol, capable of holding about 6 or 7 ounces of Water, into which we had before put 2 or 3 Spoon-fulls of that Liquor, and stopt it close with a fit Cork. The Pneumatical Vessel being empty'd, there appear'd not any change in the inclos'd Water, the Air imprison'd with it, not having the force to blow out the stopple,

ple, which event, though it were no other then we expected, was differing from what we desir'd. For we would gladly have seen what change would have appear'd in the Water upon the Bottles being suddenly unstopp'd, in a place where the ambient Body was so differing from our common Air. Wherefore we did again put in the Viol, but less strongly clos'd then formerly, though as strongly stopt as seem'd requisite on ordinary occasions: But when the Air was pump'd out of the Receiver, that within the Viol did quickly, as we expected, find or make it self little passages to get out at: as we argu'd, from this, That whereas when the Viol was put in the time before, the Water remain'd all the while perfectly free from bubbles; at this time the bottom of the Glass appear'd all cover'd with them, and they, upon the regress of the excluded Air into the Receiver, did presently flag and shrink up.

From these tryals it seem'd deducible enough, that whil'st the Viol continu'd to be well stopt, the included Water did, from the Air, shut up with it, sustain a pressure equal to that of the Atmosphere; since till the Air could get out

out of the Glaſs, there appear'd no bubbles in the Water, notwithſtanding the want of preſſure in the ambient Body.

But to be ſure to reach the chief end of our Experiment, we made uſe of this other expedient: We cauſ'd a convenient quantity of Water to be put, and Hermetically ſhut up into a Glaſs Egge, to whoſe long Neck (which was purpoſely made of an unequal thickneſs) was faſten'd to one end of a ſtring, whoſe other end was ty'd to the Cover of our Receiver, after the manner elſewhere mention'd already: Then the Egge being convey'd into the Pneumatical Veſſel, and that being evacuated, we did, by turning the braſs Stopple formerly deſcrib'd amongſt the parts of our Engine, ſo ſhorten the ſtring as to break the Glaſs; whereby liberty being given to the Air impriſon'd in the Egge, to paſs into the capacity of the Receiver, the ſudden receſs of the Air made the bubbles in a trice appear ſo numerous, and aſcend ſo ſwiftly in the Water, that their motion look'd like that of a violent ſhower of Rain; ſave that the bubbles did not, like the drops of Rain, tend downwards, but upwards, which made me reſemble this

Pha-

Phænomenon to what I have seen happen in the dissolution of Seed-Pearl in some acid *Menstruum*, in which, if a good quantity of the little Pearls be cast whole, they will at first, if the *Menstruum* be sharp enough, be carryed in swarms from the bottom to the top of the Liquor. We will adde, that without sealing up the Glass, this Experiment may be try'd in one of our smallest Receivers, for there the exsuction of the ambient Air may be perform'd so nimbly, that immediately the bubbles lurking in the Water are allow'd to display themselves, and ascend in throngs; insomuch, as having in such a Receiver try'd the Experiment with Wine (as a more spirituous Liquor) instead of Water, the Red-Wine appear'd all cover'd, with a copious, but vanishing white Froth, almost as if a Vessel full of bottl'd drink had been unwarily open'd.

IT may not a little conduce to the clearer explication of divers Points in the Doctrine of Meteors, and perhaps of some other Physiological difficulties, to discover what the Air does to the motion of those Steams or Exhalations that ascend *Experiment 29.*

cend into it, namely, Whether they mount upwards by vertue of any ſuch *poſitive levity* (as ſome Peripateticks ſpeak) acquir'd together with their Aërial nature, as inables them to pierce through part of the Atmoſphere, and over-come its reſiſtance. Or elſe, whether theſe ſteams being once raiſ'd above the Earth by their agitation, have their aſcent and ſuſtentation aloft, rather promoted then hindred by the Air: as the inferior parts of that, being thicker and heavier then the ſuperior, the ſteams can more eaſily continue for a while their agitation upwards then downwards; And afterwards are by the ſame fluidity and thickneſs of the Air, carried to and fro in it, and kept from relapſing to the Earth, as in the Sea water the ſaline parts are kept from ſubſiding by thoſe aqueous ones wherewith they are aſſociated.

We hop'd to illuſtrate this matter, by obſerving the motion of the ſmoke, proceeding from kindled or flaming Bodies in our exhauſted Receiver. But as we formerly noted, upon the exſuction of the Air, the ſmoking of thoſe Bodies preſently ceaſ'd. We had thoughts alſo of conveying into our Pneumatical Glaſs a

hot

hot Iron, with some Body easie to be dissipated into smoke set upon it, but consider'd, that neither was that way free from inconveniencies; especially this, that the hot Body would make the Imprison'd Air circulate within the Receiver, and consequently make it questionable whether the ascent of the steams would not be due to the new and acquired motion of the Air.

Wherefore I bethought my self of another way to satisfie in some measure my curiosity, to wit, by means of a certain Liquor, which I call'd to minde that some years ago I had (for a design that belongs not to our present purpose) prepar'd; which, I suppose, I shew'd Your Lordship, and which had the luck to be taken notice of by divers very Ingenious and Famous Men. For this Liquor, though most of its Ingredients be Metals, and all of them ponderous enough, is yet of that nature, that whilst the Viol wherein it is kept is stopt (how slight a Cover soever) both the Liquor and the Glass are transparent; and so is that upper half of the Glass to which the Liquor reaches not. But assoon as ever the stopple is taken out, and full access is given to the ex-

ternal Air, both the inward part of the Cork, and the Liquor it ſelf, do preſently ſend upwards, and ſcatter abroad a fume as thick and white, as if there were a quantity of Alablaſter-duſt thrown up into the Air: And this ſmoking of the Liquor laſts till my unwillingneſs to waſte it, invites me ſtop it again; and then the aſcenſion of the fumes ſuddenly ceaſes, till the Viol be again unſtop'd.

This fuming Liquor then I thought would much conduce to the diſcovery I deſir'd to make, ſince It ſav'd me the need of conveying any hot Body with it into the Receiver, and would not darken it with fumes before the time; Wherefore having ty'd to the Viol a great weight of Lead, to keep it from being lifted up by the drawing out of the Cork, and having ty'd to the ſtopple one end of a ſtring, of which the other end was made faſt to the Cover of the Pneumatical Glaſs, the Liquor was carefully cloſ'd up after the wonted manner; then the Air being diligently pump'd out, the Viol was unſtopt in the empty'd Receiver: and though immediately, upon the drawing out of the Cork, there appear'd to be as it were thrown up ſome white fumes, which

ſeem'd

seem'd to proceed from the Air before imprison'd in the Viol, and diffusing it self suddenly into the capicity of the Receiver. Yet we afterward observ'd, as we expected, That the fumes did not mount and disperse themselves as they use to do in the open Air, but that, when by reason of the agitation of the Corpuscles of the Liquor, which could not continue their motion in so narrow a space as the Viol afforded them, and were therefore reduc'd to thrust one another out of it; when, I say, by these assistances the fumes were ascended to the lip of the Viol, they mounted no higher, but ran down along the out-side of the Viol to the bottom of it; and thence along, a long and inclining piece of Lead, on which the Viol rested, like a little Stream (not very much bigger then a Swans Quill) whose nature it seemd to emulate so well, that it quitted not the Viol till it was come to the bottom of it, and then forsook it in such a manner as a stream of Water of the same bigness would have done. And this stream lasted a pretty while, and would probably have lasted longer, but that being loath to waste my Liquor, I let in at the Stop-cock a

pretty deal of the external Air; notwithstanding which, finding after a while the stream did run afresh, though, as it seem'd, not altogether so copious as before: I let as much more Air, as would, come in, and found (somewhat to my wonder) that though the stream formerly mention'd dis-appear'd, yet there appear'd not any white fumes to arise, either from the Cork, or out of the Viol it self, no not when the Cover was remov'd from the Receiver; though not onely after a while there ascended white Fumes from the Receiver: but having forthwith taken out the Viol into the open Air, it emitted white exhalations as before; and having presently after unstop'd it in an open Window, we found both it and the Cork immediatly to send forth a yet much more plentiful smoak. Though it be now divers years since this Numerical Liquor was prepared, after the manner mention'd either by *Carneiades* or *Eleutherius* (for I do not well remember which) in those Dialogues concerning Heat and Flame that have above been mention'd.

More Circumstances concerning these Fumes we might have observ'd, had we not been deterr'd by an Indisposition in point

point of health, from having much to do with steams of so dangerous a nature, as by that of the Ingredients of this Liquor these seem likely to be of.

The Reflections that may be made upon this Experiment, we have not now the leasure to prosecute, and therefore shall content our selves to recommend the several Circumstances of it to Your Lordships serious consideration; and to take notice (*en passant*) that steams in an ambient Body, or a medium thinner then themselves, may both tend downwards, and otherwise emulate the nature of a Liquor; which I therefore point at, that it may appear the less strange, if we sometimes speak of the Atmosphere as of a kinde of Liquor, in comparison of that more thin and subtle Celestial Matter that surrounds it.

And though it might perchance suffice to have on this occasion intimated thus much; yet, lest this way of speaking of the Atmosphere should be thought too bold and extravagant, I am content to borrow an Experiment of the Discourse former-

ly mention'd (touching fluidity and firmneſs) and ſubjoyn it here with alterations ſuitable to the contrivance of our Engine; and this the rather, becauſe I hope it may conduce to the diſcovery of the nature of the Atmoſphere: for which reaſon it might have been annext to what has been noted either upon the firſt, or eighteenth Experiment, but that when they were written and ſent away, it came not into my minde. The Experiment then as we try'd in our Engine, was as follows.

Experiment 30.

WE took one of the ſmall Receivers, often mention'd already, and into it we convey'd a piece of well lighted Match; and letting it remain there till it had fill'd the Receiver with ſmoak, we took it out and haſtily cloſ'd again the Receiver, that the ſmoak might not get away. Then ſtaying awhile to let theſe fumes leiſurely ſubſide, we found, as we expected, that after ſome time they ſetled themſelves in the lower half of the Receiver, in a darkiſh Body, leaving the upper half of the Receiver tranſparent, and as to ſight, full of nought but clear Air. Now to manifeſt that this ſmoak thus ſetled emulated a Li-

Liquor, we inclin'd the Engine that contain'd it, sometimes to one side, and sometimes to the other; and observ'd the smoak to keep its surface almost Horizontal, notwithstanding the stooping of the Vessel that held it, as Water or another Liquor would in the like case have done. And if by a quicker rocking of the Engine the smoke were more swiftly shaken, it would, like Water, either Vibrate to and fro from one side to the other of the Glass, or else have its surface manifestly curll'd with Waves, but preserve its self in an intire and distinct Body from the incumbent Air; and being permitted to rest awhile, would soon recover its former smooth and level *superficies*: If also the Key were turn'd and the Valve unstopp'd, so that there was a free, though but a narrow passage open'd betwixt the external Air and the cavity of the Receiver, then would some of this smoak fall down, as it were, in a stream into the subjacent Cylinder, and a proportionate quantity of the outward Air, would manifestly ascend through it into the incumbent Air, much after the same manner as if you invert a Viol with a long Neck, and well fill'd with Red-Wine, into a Glass

 full

full of fair water, you shall see the Water and Wine by degrees mingle with one another; the one falling downe as it were in little colour'd streames, and the other ascending into its room in the like curled streames, sometimes preceded by round parcels of water, which, by reason of their transparency, looke almost like bubbles. The other circumstances of this Experiment, belonging not all of them to our present purpose, we shall content our selves with taking notice of one which seemes the most important, and may illustrate and confirme some things formerly delivered. And it was, That if, when the *superficies* of our Smoke lay smooth and horizontal, a hot iron were held near the out side of the Receiver, the Neighbouring part of the included fumes (for the rest did not very much alter their former *superficies*) being rarified by the heat, would readily ascend in a large Pillar of smoke to the very top of the Receiver, yet without seeming to loose a distinct *superficies*, or to be confounded with Air; below which, upon the recess of the adventitious heat that by agitating it impell'd it upward, it would againe subside.

All which being added to the late Experiment of the smoking Liquor, and to what may be from that which has been elsewhere sayd, gather'd to the same purpose, will, I hope, keep it at least from appearing absur'd: If since we see that there is so great an inequality in the density and weight of Liquors, that water is neere 14 times thinner or lighter than Quick-silver of the same bulk, and well dephlegm'd; Spirit of Wine yet much lighter than water; we venter to speak sometimes of the Atmosphere, as if it were a peculiar kind of thin and halituous Liquor (if I may so call it) much lighter than Spirit of Wine.

To these things I know not whether it will be requisite to add, that as we lately took notice of conspicuous waves that appear'd upon the *superficies* of our agitated smoke. So some such thing may not absurdly be conjectur'd to happen on the *superficies* of the Atmosphere, by those strange ruggednesses that appeare (especially in the Spring and Fall, when exhalations and vapours are wont to ascend most plentifully) upon the Limb or Edge of the Rising and Setting Sun. I speake thus diffidently upon this occasion because I know that by the Fluctuation or

Boyling

Boyling of the Sun's own *superficies* diverse eminent Mathematicians have plausibly enough (but how truly I leave your Lordship to Judge) endeavour'd to give an Account of it. But if we will joine with those that have ascrib'd of late this *Phænomenon* to the Refraction the Sun-Beames suffer in our vapid Air; we may, as hath been intimated, promote their Doctrin by deducing from it, that probably the surface Atmosphere is oftentimes (if not alwayes) exceedingly curl'd or wav'd. And certainly it is somewhat wonderfull as well as very pleasant to behold, how, to him that looks upon the setting Sun through a long & excellent Telescope, there will not only appeare strange inequalities in the edge of it (insomuch that I have often seen it more indented than a Saw) but those inequalities will vanish in one place and presently appeare in another, and seem perfectly to move like waves succeeding and destroying one another; save that their Motion oftentimes seemes to be quickest as if in that vast sea they were carried on by a current, or at least by a tide. And this (as we else where note) appear's to the eye not only when it looks directly through the te-

lescope upon the sunne; but also when a large and well defin'd image of the sunne is by the same telescope brought into a roome and cast upon a sheet of white paper. But to insist on this were to digress; and therefore I will proceed to experiments of another kind.

IT has been admir'd by very ingenious Men, that if the exquisitly polish'd surfaces of two flat peeces of marble be so congruous to each other that from their mutuall application there will result an immediate contact, they will stick so fast together, that he that lifts up the uppermost, shall, if the undermost be not exceeding heavy, lift up that too, and sustaine it aloft in the free aire. A probable cause of this so close adhesion we have elsewhere endeavour'd to deduce from the unequall pressure of the Air upon the undermost stone; For the lower *superficies* of that stone being freely expos'd to the Air is press'd upon by it, whereas the uppermost surface, being contiguous to the superiour stone, is thereby defended from the pressure of the Air which consequently pressing the lower stone against the up- *Experiment 31.*

per,

per, hinders it from falling, as we have elſewhere more fully declar'd. Upon theſe grounds we conjectur'd that in caſe we could procure two marbles exactly ground to one another; and in caſe we could alſo ſufficiently evacuate our Receiver, the lower ſtone would, for want of the wonted and ſuſtaining preſſure of the Air, fall from the upper. But the further tryal of this Experiment we muſt, unleſs your Lordſhip think it worth Your making at *Paris*, put off till a fitter opportunity. For where we now are, we cannot procure marbles ſo exactly ground, that they will ſuſtaine one another in the Air, above a minute or two, which is a n.uch ſhorter time than the emptying of our Receiver requires. We did indeed try to make our marbles ſtick cloſe together by moiſtening their polliſhed ſurfaces with rectifi'd ſpirit of Wine, in regard that Liquor by its ſudden avolation from marble, if powr'd thereon, without leaving it moiſt or leſs ſmooth, ſeem'd unable to ſuſtaine them together after the manner of a glutinous body, and yet ſeem'd ſufficient to exclude and keep out the Air. But this we try'd to little purpoſe, for having convey'd into the Recei-

fer two black ſquare marbles (the one of two inches and a third in length or breadth, and ſomewhat more than halfe an inch in thickneſs: The other of the ſame extent, but not much above halfe ſo thick) faſten'd together by the intervention of pure Spirit of Wine; and having ſuſpended the thicker by a ſtring from the cover, we found not that the exſuction of the ambient Air would ſeparate them, though a weight amounting to four ounces were faſten'd to the lowermoſt marble to facilitate it's falling off.

I would gladly have the Experiment try'd with marble ſo well polliſh't as to need no Liquor whatſoever to make them cohere, and in a Veſſel out of which the Air may be more perfectly drawn than it was out of ours. But in the mean time though we will not determin whether the Spirit of wine did contribute to the ſtrong coheſion of theſe ſtones, otherwiſe than by keeping ev'n the ſubtl'ſt parts of the Air from getting in between them, yet it ſeemed that the not falling downe of the lowermoſt marble might without improbability be aſcrib'd to the preſſure of the Air remaining in the Receiver; which as we formerly noted having been able

to keep a Cylinder of water of above a Foot in height from falling to the bottom of the Tube, may well enough be suppos'd capable of keeping so broad a flat Marble from descending. And though this may seem a strange proof of the strength of the spring of Air, ev'n when rarified, yet it will scarce seem incredible to him that has observ'd how exceeding strong a cohesion may be made betwixt broad Bodies, onely by their immediate touching one another. A notable instance of which, I have met with in this short Narrative of the Learned *Zucchius*. *Juveni* (says he) *lacertorum suorum robur jactanti proposita semel est lamina ærea, per ansam in medio extantem apprehensam elevanda è tabula marmorea, cui optime congruebat: qui primo tanquam rem ludicram puero committendam contempsit: tum instantibus amicis manum utrámque admovens, cum luctatus diu hærentem non removisset, excusavit impotentiam, objecta perigrini & potentissimi glutinis interpositione, quo fortissime copulante nequiret divelli; donec vidit ab alio per tabulam facilimè laminam deduci, & ad extrema productam, & actam in transversum inde deportari.* But that we may learn from our own Engine,

P. Nic. Zucchius apud Schot: part 1. Mec: Hydraulopneu m.

that

that two Bodies, though they touch each other but in a ſmall part of their ſurfaces, may be made to cohere very ſtrongly, onely by this, That the Air preſſes much more forcibly upon the inferior ſuperficies of the lowermoſt Body, then upon the upper ſurface of the ſame: We will hereunto annex the following Experiment, though out of the order wherein they were made.

Experiment 32.

I remember I have, in a Diſcourſe concerning Fluidity and Firmneſs, made mention of my having, by the exſuction of the Air out of a Glaſs Veſſel, made that Veſſel take up, or ſuck up (to ſpeak in the common Language) a Body weighing divers Ounces; but our Engine affording us the opportunity of making conſiderabler Experiments of that kinde, We thought fit to make a further tryal of the force of the Atmoſphere's preſſure upwards, after the following manner.

The Receiver having been exquiſitely cloſ'd, as we have often taught already, and the Air being in a good meaſure drawn out of it, it was remov'd from off the Pump: and to the lower Branch of the

Stop-

Stop-cock; there was speedily apply'd a capering Valve of brass, such as is describ'd in the 9th fig: made fit to go with its narrower end into the cavity of the branch, and to fill the orifice of that cavity with its broader part. And that the Air might not get in at the litle intervals, left here and there between the convex surface of the stopple and the internall edge of the branch, those intervals were stop't with a little Diachylon. And to the doore, or, (if you please) that part of the Valve which was to move to and fro, and in this Experiment hung perpendicular to the Horizon, there was, at a button of brass belonging to the Valve fasten'd a broad scale wherein weights were to be put. This done the key of the Stop-cock was turn'd, and the externall Air beating like a forcible streame upon the Valve to get in there, it did suddenly both shut the Valve and keep it shut so strongly, that we had time to cast in diverse weights one after another into the Scale, till at length the weight overpowering the pressure of the Atmosphere, drew downe the Valve by the stringes that ty'd the Scale to it, and gave liberty to the outward Air to rush into the Receiver. Though another

tother time, when the Valve had but little weight hanging at it, being, by I know not what accident, drawn down beneath its former place, it was by the impetuous current of the outward Air ſuddenly impell'd up into it again, and kept there. But in the former Experiment it is remarkable, That though the Receiver were not well exhauſted, and though it leak'd whil'ſt the reſt of the Experiment was in proſecution, and though the Valve whereon the Cylinder of the Atmoſphere could preſs, were not above an Inch and a half in Diameter, yet the weight kept up by ſuction, or rather ſupported by the Air, namely the Valve, the Seal and what was caſt into it, being ſent to be weigh'd, amounted to about ten of our common Pounds, conſiſting of ſixteen Ounces apiece: So that we doubted not but that, had the Experiment been made with favorable Circumſtances, the Air endeavoring to preſs in at the Orifice of the Stop-cock, would have kept a very much greater weight from falling out of it; I ſay the Air, becauſe we found, by tryal purpoſely made, that neither the imperfect contact of the Valve and the Stop-cock, nor the Diachylon that was

employ'd to fill up the little Crannies left betwixt them, were considerable in this Experiment; by which may among other things appear, that I did not without cause in the above-nam'd Discourse touching Fluidity and Firmness, ascribe a great force, ev'n to such Pillars of Air as may be suppos'd to begin at the top of the Atmosphere, and recoyling from the ground to terminate on the Bodies on which they press: since in the present Experiment such a weight was supported by so slender a Cylinder of Air, rebounding from the Earth to the Valve whereon it did bear.

Experiment 33.

BUt in regard we have not yet been able to empty so great a Vessel as our Receiver, so well as we can the Cylinder it self; our Pump alone may afford us a nobler instance of the force of the Air we live in, insomuch, that by help of this part of our Engine, we may give a pretty near ghess at the strength of the Atmosphere, computed as a weight. And the way may be this; First, the Sucker being brought to move easily up and down the Cylinder, is to be impell'd to the top

of it: Then the Receiver muſt be taken off from the Pump, that the upper Orifice of the Cylinder remaining open, the Air may freely ſucceed the Sucker, and therefore readily yield to its motion downward. This done, there muſt be faſten'd to one of the Iron Teeth of the Sucker, ſuch a weight as may juſt ſuffice to draw it to the bottom of the Cylinder. And having thus examin'd what weight is neceſſary to draw down the Sucker, when the Atmoſphere makes no other then the ordinary reſiſtance of the Air againſt its deſcent; the Sucker muſt be again forc'd to the top of the Cylinder, whoſe upper Orifice muſt now be exactly cloſed; and then (the firſt weight remaining) we eaſily may, by hanging a Scale to the abovemention'd Iron (that makes part of the Sucker) caſt in known weights ſo long, till in ſpight of the reluctancy of the Atmoſphere the Sucker be drawn down. For to theſe weights in the Scale, that of the Scale it ſelf being added, the ſum will give us the weight of a Column of Air, equal in Diameter to the Sucker, or to the cavity of the Cylinder; and in length to the heighth of the Atmoſphere.

According to this method we did, ſince

the writing of the laſt Experiment, attempt to meaſure the preſſure of the Atmoſphere, but found it more difficult then we expected, to perform it with any accurateneſs; for though by the help of the *Manubrium* the Sucker moved up and down with ſo much eaſe, that one would have thought that both its convex ſurface, and the concave one of the Cylinder were exquiſitely ſmooth, & as it were ſlippery; yet when the Sucker came to be moved onely with a dead weight or preſſure (that was not (like the force of him that pump'd) intended as occaſion required) we found that the little rufneſſes, or other inequalities, and perhaps too, the unequal preſſure of the Leather againſt the cavity of the Cylinder, were able now and then to put a ſtop to the deſcent or aſcent of the Sucker, though a very little external help would eaſily ſurmount that impediment; and then the Sucker would, for a while, continue its formerly interrupted motion, though that aſſiſtance were withdrawn. But this diſcouragement did not deterre us from proſecuting our Experiment, and endeavoring, by a careful trial, to make it as inſtructive as we could. We found then that a Leaden Weight,

of

of 28 pounds (each conſiſting of ſixteen Ounces) being faſtned to one of the teeth of the Sucker, drew it down ſlowly enough; when the upper Orifice of the Cylinder was left open, though by the help of Oyl and Water, and by the frequent moving the Sucker up and down with the *Manubrium*, its motion in the Cylinder had been before purpoſely facilitated. This done, the upper Orifice of the Cylinder was very carefully and cloſely ſtopp'd, the Valve being likewiſe ſhut with its wonted Stopple well oyl'd, after the Sucker had been again impell'd up to the top of the Cylinder. Then to the precedent twenty eight pound, we added a hundred and twelve pounds more; which forcing down the Sucker, though but leiſurely, we took off the twenty eight pound weight; and being unable to procure juſt ſuch weights as we would have had, we hung on, inſtead of it, one of fourteen pound, but found that, with the reſt, unable to carry down the Sucker. And to ſatisfie our ſelves, and the Spectators, that it was the reſiſtance of the ambient Air that hinder'd the deſcent of ſo great a weight, after that we had try'd that upon unſtopping the Valve, and

thereby opening an acceſs to the external Air, the Sucker would be immediately drawn down: After this, I ſay, we made this further Experiment, That having by a Man's ſtrength forcibly depreſſ'd the Sucker to the bottom of the Cylinder, and then faſtned weights to the above-named Iron that makes part of that Sucker, the preſſure of the external Air finding little or nothing in the cavity of the evacuated Cylinder to reſiſt it, did preſently begin to impell the Sucker, with the weights that clogg'd it, towards the upper part of the Cylinder, till ſome ſuch accidental Impediment as we formerly mention'd, check'd its courſe; and when that rub, which eaſily might be, was taken out of the way, it would continue its aſcent to the top, to the no ſmall wonder of thoſe By-ſtanders, that could not comprehend how ſuch a weight could aſcend, as it were, of it ſelf; that is, without any viſible force, or ſo much as Suction to lift it up. And indeed it is very conſiderable, that though poſſibly there might remain ſome particles of Air in the Cylinder, after the drawing down of the Sucker; yet the preſſure of a Cylinder of the Atmoſphere, ſomewhat leſs then

three

three Inches in Diameter (for, as it was ſaid in the deſcription of our Engine, the cavity of the Cylinder was no broader) was able, uncompreſſ'd, not only to ſuſtain, but even to drive up a weight of an hundred and odde pounds: for beſides the weight of the whole Sucker it ſelf; which amounts to ſome pounds, the weights annexed to it made up a hundred and three pounds, beſides an Iron Bar, that by conjecture weighed two pounds more; and yet all theſe together fall ſomewhat ſhort of the weight which we lately mention'd, the reſiſtance of the Air to have held ſuſpended in the cavity of the Cylinder.

And though (as hath been already acknowledg'd) we cannot, peradventure, obtain by the recited means ſo exact an account as were to be wiſh'd, of what we would diſcover: Yet, if it ſerve us to ground Conjectures more approaching to the Truth, then we have hitherto met with, I hope it will be conſider'd (which a famous Poet judiciouſly ſays)

Eſt quoddam prodire tenus, ſi non datur ultra.

Peradventure it will not be imperti-

nent to annex to the other Circumſtances that have been already ſet down concerning this Experiment, That it was made in Winter, in Weather neither Froſty nor Rainy, about the change of the Moon, and at a place whoſe latitude is near about 51^{d} and a half: For perhaps the force or preſſure of the Air may vary, according to the Seaſons of the Year, the temperature of the Weather, the elevation of the Pole, or the phaſes of the Moon; all, or even any of them ſeeming capable to alter either the heighth or conſiſtence of the incumbent Atmoſphere: And therefore it would not be amiſs if this Experiment were carefully tryd at ſeveral times and places, with variety of Circumſtances. It might alſo be try'd with Cylinders of ſeveral Diameters, exquiſitely fitted with Suckers, that we might know what proportion ſeveral Pillars of the Atmoſphere bear, to the Weights they are able to ſuſtain or lift up; and conſequently, whether the increaſe or decrement of the reſiſtance of the ambient Air, can be reduc'd to any regular proportion to the Diameters of the Suckers: Theſe, and divers other ſuch things which may be try'd with this Cylinder, might moſt of

them

them be more exactly try'd by the Torricellian Experiment, if we could get Tubes so accurately blown and drawn, that the Cavity were perfectly Cylindrical.

To dwell upon all the ſeveral Reflections, that a ſpeculative Wit might make upon this and the foregoing Experiment, (I mean the thirty third and thirty ſecond) would require almoſt a Volume; whereas our occaſions will ſcarce allow us time to touch upon three or four of the chief Inferences that ſeem deducible from them, and therefore we ſhall content our ſelves to point at thoſe few.

And firſt, as many other *Phænomena* of our Engine, ſo eſpecially, the two lately mention'd Experiments, ſeem very much to call in queſtion the receiv'd Opinion of the Nature or Cauſe of Suction. For 'tis true indeed, that when men ſuck, they commonly uſe ſome manifeſt endeavour by a peculiar motion of their Mouthes, Cheſts, and ſome other conſpiring parts, to convey to them the body to be ſuckt in. And hence perhaps they have taken occaſion, to think that in all

Suction

Suction there must be some Endeavour or motion in the sucking to attract the sucked Body. But in our last Experiment it appeares not at all how the upper part of the empty'd Cylinder that remaines moveless all the while, or any part of it, does at all endeavour to draw to it the depressed Sucker and the annex'd weights. And yet those that behold the ascention of the Sucker, without seriously considering the cause of it, doe readily conclude it to be rays'd by something that powerfully Sucks or attracts it, though they see not what that may be or where it lurks. So that it seemes not absolutely necessary to Suction, that there be in the Body, which is said to suck, an endeavour or motion in order thereunto, but rather that Suction may be at least for the most part reduc'd to Pulsion, and its effects ascrib'd to such a pressure of the neighboring air upon those Bodies (whther aërial, or of other Natures) that are contiguous to the Body that is sayd to attract them, as is stronger than that Substance which possesses the cavity of that sucking Body is able to resist. To object here, that it was some particles of Air remaining in the empty'd Cylin-

der that attracted this weight to obviate a *Vacuum*, will ſcarce be ſatisfactory; unleſs it can be cleerly made out by what litle hooks, or other grappling Inſtruments, the internal Air could take hold of the Sucker; how ſo litle of it obtain'd the force to lift up ſo great a weight; and why alſo, upon the letting in of a litle more Air into one of our evacuated Veſſels, the attraction is, inſtead of being ſtrengthen'd, much weaken'd, though, if there were danger of a *Vacuum* before, it would remain, notwithſtanding this ingreſs of a little Air. For that ſtill there remain'd in the capacity of the exhauſted Cylinder ſtore of little rooms, or ſpaces empty or devoid of Air, may appear by the great violence wherewith the air ruſhes in, if any way be open'd to it. And that 'tis not ſo much the decrement of the *Vacuum* within the cavity of the veſſel that debilitates the attraction, as the ſpring of the included air (whoſe preſence makes the decrement) that does it by reſiſting the preſſure of the external Air, ſeems probable, partly from the Diſability of vacuities, whether greater or leſſer, to reſiſt the preſſure of the Air; and partly by ſome of the *Phænomena* of our Ex-

periments, and particularly by this Circumstance of the three and Thirtieth, that the Sucker was by the pressure of the Ambient Air impell'd upwards, with its weight hanging at it, not only when it was at the bottome of the Cylinder, and consequently left a great *Vacuum* in the cavity of it; but when the Sucker had been already impel'd almost to the top of the Cylinder, and consequently, when the *Vacuum* that remain'd was become very litle in comparison of that which preceded the beginning of the Sucker's ascention.

In the next place, these Experiments may teach us, what to judge of the vulgar Axiom receiv'd for so many Ages as an undoubted Truth in the Peripatetick Schools; That Nature abhorres and flys a *Vacuum*, and that to such a degree, that no humane power (to go no higher) is able to make one in the Universe; wherein Heaven and Earth would change places, and all its other Bodyes rather act contrary to their own Nature, than suffer it. For, if by a *Vacuum* we will understand a place perfectly devoid of all corporeal Substance, it may be indeed then, as we formerly noted, be plausibly enough maintain'd, that there is

no

no such thing in the world; but that the generality of the Plenists, (especially till of late yeares some of them grew more wary) did not take a *Vacuum* in so strict a Sense, may appear by the Experiments formerly, and ev'n to this Day imploy'd by the Deniers of a *Vacuum*, to prove it impossible that there can be any made. For when they alleadge (for Instance) that when a man sucks Water through a long Pipe, that heavy Liquor, contrary to its Nature, ascends into the Sucker's mouth, only, to fill up that room made by the Dilatation of his Brest and Lungs, which otherwise will in part be empty. And when they tell us, that the reason why if a long Pipe exactly clos'd at one end be fill'd top-ful of Water, and then inverted, no Liquor will fall out of the open Orifice; Or, to use a more familiar Example, when they teach, that the cause why in a Gardiner's watering Pot shap'd conically, or like a Sugar-Loaf fill'd with Water, no Liquor fals down through the numerous holes at the bottome, whilst the Gardiner keeps his Thumb upon the Orifice of the litle hole at the top; and no longer, must be that if in the case proposed the Water

ſhould deſcend, the Air being unable to ſucceed it, there would be left at the upper and deſerted part of the Veſſel a *Vacuum*, that would be avoided if the hole at the top were open'd. When (I ſay) they alleadge ſuch Experiments, the Tendency of them ſeems plainly to import, that they mean, by a *Vacuum*, any ſpace here below that is not fill'd with a viſible body, or at leaſt with Air; though it be not quite devoy'd of all Body whatſoever. For why ſhould Nature, out of her deteſtation of a *Vacuum*, make Bodies act contrary to their own Tendency, that a place may be fill'd with Air, if its being ſo were not neceſſary to the avoiding of a *Vacuum*.

Taking then a *Vacuum* in this vulgar and obvious ſence, the common opinion about it ſeems lyable to ſeveral Exceptions, whereof ſome of the chief are ſuggeſted to us by our Engine.

It will not eaſily then be intelligibly made out, how hatred or averſation, which is a paſſion of the Soule, can either for a *Vacuum*, or any other object, be ſuppos'd to be in Water, or ſuch like inanimate Body, which cannot be preſum'd to know when a *Vacuum* would enſue; if they

they did not bestirre themselves to prevent it, nor to be so generous as to act contrary to what is most conducive to their own particular preservation for the publique good of the Universe. As much then of intelligible and probable Truth, as is contain'd in this Metaphoricall Expression, seems to amount but to this; That by the Wise Author of Nature (who is justly sayd to have made all things in number, weight, and measure,) the Universe, and the parts of it, are so contriv'd, that it is as hard to make a *Vacuum* in it, as if they studiously conspir'd to prevent it. And how far this it selfe may be granted, deserves to be further consider'd.

For in the next place, our Experiments seem to teach, that the supposed Aversation of Nature to a *Vacuum* is but accidental, or in consequence partly of the Weight and Fluidity, or, at least, Fluxility of the Bodies here below; and partly, and perhaps principally, of the Spring of the air, whose restless endeavor to expand it selfe every way, makes it either rush in it selfe, or compel the interpos'd bodys into all spaces, where it finds no greater resistance than it can surmount. And that

that in those motions which are made *ob fugam Vacui* (as the common phrase is) Bodys act without such generosity & Consideration, as is wont to be ascrib'd to them, is apparent enough in our 32[d] Experiment, where the torrent of Air, that seem'd to strive to get into the Empty'd Receiver, did plainly prevent its own Designe, by so impelling the Valve, as to make it shut the only Orifice the Air was to get in at. And if afterwards either Nature, or the internal Air, had a designe the external Air should be attracted, they seem'd to prosecute very unwisely by continuing to suck the Valve so strongly, when they found that by that Suction the Valve it selfe could not be drawn in: Whereas by forbearing to suck, the Valve would by it's own weight have fall'n down, and suffer'd the excluded Air to returne freely, and to fill again the exhausted Vessel.

And this minds me to take notice of another deficiency, pointed at by our Experiments in the common Doctrine of those Plenists we reason with; for many of those unusual motions in Bodies, that are sayd to be made to escape a *Vacuum*, seem rather made to fill it. For why,

to

to instance in our newly mention'd Experiment, assoon as the Valve was depress'd by the weight we hung at it, should the Air so impetuously and copiously rush into the cavity of the Receiver; if there were before no vacant room there to receive it? and if there were, then all the while the Valve kept out the Air, those litle spaces in the Receiver, which the corpuscles of that Air afterwards fill'd, may be concluded to have remain'd empty. So that the seeming violence, imploy'd by Nature on the occasion of the evacuating of the Vessel, seems to have come too late to hinder the making of Vacuities in the Receiver, and only to have, assoon as we permitted, fill'd up with Air those that were already made.

And as for the Care of the Publique Good of the Universe ascrib'd to dead and stupid Bodies, wee shall only demand, why in our 19th Experiment, upon the Exsuction of the ambient Air, the Water deserted the upper half of the Glass-Tube; and did not ascend to fill it up, till the external Air was let in upon it: whereas by its easy and sudden regaining that upper part of the Tube, it

appear'd both that there was there much ſpace devoid of Air, and that the Water might with ſmall or no reſiſtance have aſcended into it, if it could have done ſo without the impulſion of the readmitted Air; which, it ſeems, was neceſſary to mind the Water of its formerly neglected Duty to the Univerſe.

Nay, for ought appeares, ev'n when the excluded Air, aſſoon as 'twas permitted, ruſht violently into our exhauſted Receiver, that flowing in of the Air proceeded rather from the determinate Force of the Spring of the neighbouring Air, then from any endeavour to fill up, much leſs to prevent vacuity's. For though when as much Air as will, is gotten into our Receiver our preſent Opponents take it for granted that it is full of Air; yet if it be remembred that when we made our 17th Experiment we crouded in more Air to our Receiver than it uſually holds; and if we alſo conſider (which is much more) the Air of the ſame conſiſtence with that in our Receiver may in wind-guns, as is known, and as we have try'd, be compreſſed at leaſt into halfe its wonted room (I ſay at leaſt, because ſome affirme, that the Air

may

may be thrust into an 8th, or a yet smaller part of its ordinary extent) it seems necessary to admit either a notion of condensation & rarefaction that is not intelligible, or that in the capacity of our Receiver when presum'd to be full of Air, there yet remain'd as much of space as was taken up by all the aërial corpuscles unpossessed by the Air. Which seemes plainly, to infer that the Air that rush'd into our empty'd vessel did not doe it precisely to fill up the Vacuities of it, since it left so many unfill'd, but rather was thrust in by the pressure of the contiguous Air; which as it could not, but be always ready to expand it selfe, where it found least resistance, so was it unable to fill the Receiver any more, then until the Air within was reduc'd to the same measure of Compactness with that without.

We may also from our two already often mention'd Experiments further deduce, that, (since Natures hatred of a *Vacuum* is but Metaphorical and Accidental, being but a consequence or result of the pressure of the Air and of the Gravity, and partly also of the Fluxility of some other bodies) The power shee makes use of to hinder a Vacuum, is not

(as we have elſe-where alſo noted) any ſuch boundleſs thing as men have been pleas'd to imagine. And the reaſon, why in the former Experiments, mentioned in favour of the Pleniſts, Bodies ſeem to forget their own Natures to ſhun a *Vacuum*, ſeems to be but this; That in the alleadged caſes the weight of that Water that was either kept from falling or impell'd up, was not great enough to ſurmount the preſſure of the contiguous Air; which, if it had been, the Water would have ſubſided, though no Air could have ſucceeded. For not to repeat that Experiment of Monſieur *Paſchal* (formerly mention'd to have been try'd in a Glaſs exceeding 32 Foot) wherein the inverted Pipe being long enough to contain a competent weight of Water, that Liquor freely ran out at the lower Orifice: Not to mention this (I ſay) we ſaw in our nineteenth Experiment, that when the preſſure of the ambient Air was ſufficiently weaken'd, the Water would fall out apace at the Orifice even of a ſhort Pipe, though the Air could not ſucceed into the room deſerted by it. And it were not amiſs if tryal were made on the tops of very high Mountains, to diſcover with

what

what eaſe a *Vacuum* could be made near the confines of the Atmoſphere, where the Air is probably but light in compariſon of what it is here below. But our preſent (three and thirtieth) Experiment ſeems to manifeſt, not onely that the power, exerciſ'd by Nature, to ſhun or repleniſh a *Vacuum*, is limited, but that it may be determin'd even to Pounds and Ounces: Inſomuch that we might ſay, ſuch a weight Nature will ſuſtain or will lift up to reſiſt a *Vacuum* in our Engine; but if an Ounce more be added to that weight, it will ſurmount Her ſo much magnifi'd deteſtation of Vacuities. And thus, My Lord, our Experiments may not onely anſwer thoſe of the Pleniſts; but enable us to retort their Arguments againſt themſelves: ſince, if that be true which they alleadge, that, when Water falls not down according to its nature, in a Body wherein no Air can ſucceed to fill up the place it muſt leave, the ſuſpenſion of the Liquor is made *Ne detur Vacuum*, (as they ſpeak) it will follow, that if the Water can be brought to ſubſide in ſuch a caſe, that deſerted ſpace may be deem'd empty, according to their own Doctrine; eſpecially, ſince Nature (as

they would perſwade us) beſtirs her ſelf ſo mightily to keep it from being deſerted.

I hope I ſhall not need to reminde Your Lordſhip, that I have all this while been ſpeaking of a *Vacuum*, not in the ſtrict and Philoſophical ſenſe, but in that more obvious and familiar one that has been formerly declar'd.

And therefore I ſhall now proceed to obſerve in the laſt place, that our 33^d Experiment affords us a notable proof of the unheeded ſtrength of that preſſure which is ſuſtain'd by the Corpuſcles of what we call the free Air, and preſume to be uncompreſſ'd. For, as fluid and yielding a Body as it is, our Experiment teaches us, That ev'n in our Climate, and without any other compreſſion then what is (at leaſt here below) Natural, or (to ſpeak more properly) ordinary to it, it bears ſo ſtrongly upon the Bodies whereunto it is contiguous, that a Cylinder of this free Air, not exceeding three Inches in Diameter is able to raiſe and carry up a weight, amounting to between ſixteen and ſeventeen hundred Ounces. I ſaid, even

even in our Climate, because that is temperate enough; and as far as my observations assist me to conjecture, the Air in many other more Northern Countries may be much thicker, and able to support a greater weight: which is not to be doubted of, if there be no mistake in what is Recorded concerning the *Hollanders*, that were forc'd by the Ice to Winter in *Nova Zembla*, namely, That they found there so condens'd an Air, that they could not make their Clock goe, ev'n by a very great addition to the weights that were wont to move it.

Aere frigido existente tardius moventur Automata quàm aere calido, adeo quidem ut Automaton quod Belgæ in Nova Zembla agentes in ædibus suis collocaverunt, omnino à motu cessaverit: etsi multo majus pondus ei addidissent quam antea ferre solebat. Varenius Geo: Generat lib. III. Propo: 7 pag. 648.

I suppose Your Lordship will readily take notice, that I might very easily have discoursed much more fully and accuratly then I have done, against the common opinion touching Suction, and touching natures hatred of a *Vacuum*. But I was willing to keep my self to those considerations touching these matters, that might be verifi'd by our Engine it self, especially, since, as I said at first, it would take up too much time to insist particularly upon all the Reflections that may be made even upon our two last Experiments. And therefore,

passing to the next, I shall leave it to your Lordship to consider how far these tryals of ours will either confirm or disfavor the new Doctrine of several eminent Naturalists, who teach, That in all motion there is necessarily a Circle of Bodies, as they speak, moving together; and whether the Circles in such motion be an Accidental or Consequential thing or no.

Experiment 34. T Is a known thing to those that are conversant in the Hydrostaticks, That two Bodies which in the Air are of equal weight, but of unequal bulk, as Gold, for instance and Iron, being afterwards weighed in Water, will lose their *Æquilibrium* upon the change of the ambient Body, so that the Gold will sink lower then the Iron; which, by reason of its greater bulk, has more Water to lift or displace, that it may sink. By Analogy to this Experiment, it seem'd probable, that if two weights did in our Engine ballance each other, when the Glass was full of Air; upon the exsuction of a great part of that Air, so notable a change in the consistence of the ambient Body, would

would make them lose their *Æquilibrium*.

But being desirous at the same time to make a tryal, for a certain Design that needs not here be mention'd, we took for one of our weights a dry Bladder, strongly tyed at the Neck, and about half fill'd with Air (that being a weight both slight, and that would expand it self in the evacuated Glass) and fastning that to one part of our formerly mention'd exact ballance (which turns with the 32d part of a Grain) we put a Metalline counterpoise into the opposite Scale; and so the two weights being brought to an *Æquilibrium*, the ballance was convey'd into the Receiver, and suspended from the Cover of it.

But before we proceed further, we must note, That presently after the laying on of the Cover, the Bladder appear'd to preponderate, whereupon the Scales being taken out, and reduc'd very near to an *Æquilibrium*, yet so, that a little advantage remain'd on that side to which the Metalline weight belong'd; they were again let down into the Receiver, which was presently made fast with Plaister, and a hot Iron: Soon after which, before the Pump

was

was employ'd, the Bladder seem'd again a little to preponderate. Afterwards the Air in the Glass being begun to be drawn out, the Bladder began (according to the formerly mention'd Observations) to expand it self, and manifestly to outweigh the opposite weight, drawing down the Scale to which it was fastned very much beneath the other, especially when the Air had swell'd it to its full extent.

This done, we very leisurely let in the external Air; and observ'd, that upon the flagging of the Bladder, the Scale whereto it was fastned, not onely by degrees return'd to an *Æquilibrium* with the other, but at length was a little outweighed by it.

But because we suspected there might have interven'd some unheeded Circumstance in this last part of the Experiment, we would not presently take out the Scales, nor meddle with the Cover, but leaving things as they were, we perceiv'd, that after a little while the Bladder began again to preponderate, and by degrees to sink lower and lower for divers hours; wherefore, leaving the Vessel clos'd up all night, we repair'd to it

next Morning, and found the Bladder fallen yet lower. As if the very substance of it, had imbibed some of the moisture wherewith the Air (the Season being very rainy) did then abound: As Lute-strings, which are made likewise of the Membranous parts of Guts, strongly wreath'd, are known to swell so much, oftentimes as to break in rainy and wet weather. Which conjecture is the more to be regarded, because congruously unto it one of the company having a little warm'd the Bladder, found it then lighter then the opposite weight. But this must be look'd upon as a bare conjecture, till we can gain time to make further tryals about it. In the mean while we shall adde, that without removing the Scales or the Cover of the Receiver, we again caus'd the Air to be drawn out (the weather contiing very moist) but found not any manifest alteration in the ballance; whether because the *Æquilibrium* was too far lost to let a small change appear, we determine not.

But to make the Experiment with a Body less apt to be altered by the temperature of the Air, then was the Bladder; we brought the Scales again to an *Æquilibrium*

librium with two weights, whereof the one was of Lead, the other of Cork. And having evacuated the Receiver, we observed, that both upon the exsuction, and after the return of the Air, the Cork did manifestly preponderate, and much more a while after the Air had been let in again, then whilst it was kept out. Wherefore, in the room of the Cork, we substituted a piece of Char-coal, as less likely to imbibe any moisture from the Air, but the event proved much the same with that newly related: So that this Experiment seems more liable to Casualties then any, excepting one we have made in our Engine. And as it is difficult to prevent them, so it seems not very easie to discover the causes of them, whereof we shall therefore at present forbear mentioning our Conjectures.

Experiment 35. SOme Learned Mathematicians have of late ingeniously endeavored to reduce Filters to *Siphons*; but still the true cause of the ascension of Water, and other Liquors, both in *Siphons* and in Filtration, needing (for ought we have yet found) a clearer Discovery and Explication, we

were

were desirous to try whether or no the pressure of the Air might reasonably be suppos'd to have either the principal, or at least a considerable Interest in the raising of those Liquors. But because we found that we could not yet so evacuate our Receiver, but that the remaining Air, though but little in comparison of the exhausted, would be able to impell the the Water to a greater height then is usual in ordinary Filtrations: we resolved, instead of a List of Cotton, or the like Filtre, to make use of a *Siphon* of Glass, delineated in the third Figure, consisting of three pieces, two straight, and the third crooked to joyn them together; whose Junctures were diligently clos'd, that no Air might finde entrance at them. One of the Legs of this *Siphon* was (as it should be) somewhat longer then the other, and was pervious at the bottom of it onely, by a hole almost as slender as a hair, that the Water might but very leasurely drop out of it, lest it should all run out before the Experiment were compleated. The other and shorter Leg of the *Siphon* was quite open at the end, and of the same widenesse with the

rest

rest of the Pipe, whose bore was about $\frac{1}{7}$ of an Inch. The whole Siphon made up of these several pieces put together, was design'd to be about a Foot and a half long; that the remaining Air, when the Vessel was exhausted after the wonted manner, might not be able to impell the Water to the top of the *Siphon*; which being inverted, was fill'd with Water, and of which the Shorter leg being let down two or three Inches deep into a Glass Vessel full of Water, and the upper parts of it being fasten'd to the inside of the Cover of the Receiver, we proceeded to close first, and then to empty the Vessel.

The effect of the tryal was this, that till a pretty quantity of Air had been drawn out, the Water dropp'd freely out at the lower end of the lower leg of the *Siphon*, as if the Experiment had been performed in the free Air. But afterwards, the Bubbles (as had been apprehended) began to disclose themselves in the Water, and ascending to the top of the *Siphon*, imbodyed themselves there into one, which was augmented little by little by the rising of other bubbles that from time to time broke into it, but much

more

more by its own dilatation, which encreas'd proportionably to the exsuction that was made of the Air out of the Receiver. So that at length the Water in the shorter Leg of the *Siphon* was reduc'd partly by the extraction of the ambient Air, and partly by the expansion of the great Bubble at the upper part of the *Siphon*, to be but about a Foot high, if so much; whereby it came to pass, that the course of the Water in the *Siphon* was interrupted, and that which remain'd in the longer Leg of it, continu'd suspended there without dropping any longer. But upon the turning of the Stop-cock, the outward Air (being let into the Receiver) got into the *Siphon* by the little hole at which the Water formerly dropt out; and traversing all the incumbent Cylinder of Water, in the form of Bubbles, joyn'd it self with that Air that before possess'd the top of the *Siphon*.

To prevent the inconveniences arising from these Bubbles, two Glass Pipes, like the former, were so placed, as to terminate together in the midst of the Belly of a Glass Viol, into whose Neck they were carefully fastned with Cement; and then

then both the Viols and the Pipes being (which was not done without difficulty) totally fill'd with Water, the *Siphon* describ'd in the fifth Figure, was plac'd with its shorter Leg in the Glass of Water, as formerly; and the Experiment being prosecuted after the same manner, much more Air then formerly was drawn out, before the Bubbles disclosing themselves in the Water were able to disturb the Experiment; because that in the capacity of the Viol there was room enough for them to stretch themselves, without depressing the Water below the ends of the Pipes; and, during this time, the Water continued to drop out of the propending Leg of the *Siphon*. But at length the Receiver being very much empty'd, the passage of the Water through the *Siphon* ceas'd, the upper ends of the Pipes beginning to appear a little above the remaining Water in the Viol, whose dilated Air appear'd likewise to press down the Water in the Pipes, and fill the upper part of them. And hereby the continuity of the Water, and so the Experiment it self being interrupted, we were invited to let in the Air again, which, according to its various proportions of

pressure

pressure to that of the Air in the Viol and the Pipes, did for a good while exhibite a pleasing variety of *Phænomena*, which we have not now the leisure to recite. And though upon the whole matter there seem'd little or no cause to doubt, but that, if the Bubbles had not disturb'd the Experiment, it would manifestly enough have appear'd that the course of Water through *Siphons* depends upon the pressure of the Air: yet we resolv'd, at our next leisure and conveniency, to try the Experiment again, with a quantity of Water before freed from Bubbles by the help of the same Engine.

This occasion I have had to take notice of *Siphons*, puts me in minde of an odde kinde of *Siphon* that I caus'd to be made a pretty while ago; and which has been since, by an Ingenious Man of Your acquaintance, communicated to divers others. The occasion was this, An eminent Mathematician told me one day, that some inquisitive French Men (whose Names I know not) had observ'd, That, in case one end of a slender and perforated Pipe of Glass be dipt in Water, the Li-

quor will aſcend to ſome height in the Pipe, though held perpendicular to the plain of the Water. And, to ſatisfie me that he miſ-related not the Experiment, he ſoon after brought two or three ſmall Pipes of Glaſs, which gave me the opportunity of trying it: though I had the leſs reaſon to diſtruſt it, becauſe I remember I had often in the long and ſlender Pipes of ſome Weather Glaſſes, which I had cauſ'd to be made after a ſomewhat peculiar faſhion, taken notice of the like aſcenſion of the Liquor, though (preſuming it might be caſual) I had made but little reflection upon it. But after this tryal, beginning to ſuppoſe, that though the Water in theſe Pipes that were brought me, riſe not above a quarter of an Inch, (if near ſo high) yet, if the Pipes were made ſlender enough, the Water might riſe to a very much greater height; I cauſ'd ſeveral of them to be, by a dexterous Hand, drawn out at the flame of a Lamp, in one of which that was almoſt incredibly ſlender, we found that the Water aſcended (as it were of it ſelf) five Inches by meaſure, to the no ſmall wonder of ſome famous Mathematicians, who were Spectators of ſome of theſe

Ex-

Experiments. And this height the Water reach'd to, though the Pipe were held in as erected a posture as we could: For if it were inclin'd, the Water would fill a greater part of it, though not rise higher in it. And we also found, that when the inside of the Pipe was wetted beforehand, the Water would rise much better then otherways: But we caus'd not all our slender Pipes to be made straight, but some of them crooked, like *Siphons*: And having immers'd the shorter Leg of one of these into a Glass that held some fair Water, we found, as we expected, that the Water arising to the top of the *Siphon*, though that were high enough, did of it self run down the longer Leg, and continue running like an ordinary *Siphon*. The cause of this ascension of the Water, appear'd to all that were present so difficult, that I must not stay to enumerate the various Conjectures that were made at it, much less to examine them; especially, having nothing but bare Conjectures to substitute in the room of those I do not approve. We try'd indeed, by conveying a very slender Pipe and a small Vessel of Water into our Engine, whether or no the Exsuction of the ambient

Air would aſſiſt us to finde the cauſe of the aſcenſion we have been ſpeaking of: But though we imploy'd red Wine inſtead of Water, yet we could ſcarce certainly perceive thorow ſo much Glaſs, as was interpoſ'd betwixt our Eyes and the Liquor, what happen'd in a Pipe ſo ſlender, that the redneſs of the Wine was ſcarce ſenſible in it. But as far as we could diſcern, there happen'd no great alteration to the Liquor: which ſeem'd the leſs ſtrange, becauſe the Spring of that Air that might depreſs the Water in the Pipe, was equally debilitated with that which remain'd to preſs upon the ſurface of the Water in the little Glaſs. Wherefore, in favor of his Ingenious Conjecture who aſcrib'd the *Phænomenon*, under conſideration to the greater preſſure made upon the Water by the Air without the Pipe, then by that within it, (where ſo much of the Water (conſiſting perhaps of Corpuſcles more pliant to the internal ſurfaces of the Air) was contiguous to the ſides) it was ſhown, that in caſe the little Glaſs Veſſel that held the Water, of which a part aſcended into the ſlender Pipe, were ſo cloſ'd, that a Man might with his mouth ſuck the Air out of it, the Water would im-

immediately ſubſide in the ſmall Pipe. And this would indeed infer, that it aſcended before onely by the preſſure of the incumbent Air: But that it may (how juſtly I know not) be objected, that peradventure this would not happen, in caſe the upper end of the Pipe were in a *Vacuum*: And that 'tis very probable the Water may ſubſide, not becauſe the preſſure of the internal Air is taken off by Exſuction, but by reaſon of the Spring of the external Air, which impels the Water it findes in its way to the Cavity deſerted by the other Air, and would as well impell the ſame Water upwards, as make it ſubſide, if it were not for the accidental poſture of the Glaſſes. However, having not now leiſure to examine any further this Matter, I ſhall onely minde Your Lordſhip, that if You will proſecute this Speculation, it will be pertinent to finde out likewiſe, Why the ſurface of Water (as is manifeſt in Pipes) uſes to be concave, being depreſs'd in the middle, and higher on every ſide? and Why in Quick-ſilver on the contrary, not onely the ſurface is wont

to be very convex, or ſwelling, in the middle; but if you dip the end of a ſlender Pipe in it, the ſurface of the Liquor (as 'tis call'd) will be lower within the Pipe, then without. Which *Phænomena*, whether, and how far, they may be deduc'd from the Figure of the Mercurial Corpuſcles, and the Shape of the Springy Particles of the Air, I willingly leave to be conſider'd.

Experiment 36. SEveral ways we have met with propos'd, partly by the excellent *Galileo*, and partly by other ingenious Writers, to manifeſt that the Air is not devoid of weight; ſome of theſe, require the previous abſence of the Air to be weighed; and others, the violent condenſation of it. But if we could lift a pair of Scales above the Atmoſphere, or place them in a *Vacuum*, we might there weigh a parcel of Air it ſelf, as here we do other Bodies in the Air, becauſe it would there be heavier then that which ſurrounds it, as are groſſer Bodies we commonly weigh, then the medium or ambient Air. Wherefore, though we have above declin'd to affirm, that our Receiver, when emptyed,

ed, deserves the name of a true *Vacuum*, and though we cannot yet perfectly free it from Air it self, yet we thought fit to try how far the Air would manifest its gravity in so thin a medium, as we could make in our Receiver, by evacuating it. We caus'd then to be blown at the Flame of a Lamp, a Glass-bubble of about the bigness of a small Hen-egge, and of an Oval form, save that at one end there was drawn out an exceeding slender Pipe, that the Bubble might be seal'd up, with as little rarifaction as might be, of the Air included in the great or ovall Cavity of it. This Glass being seal'd, was fastened to one of the Scales of the exact pair of Ballances formerly mention'd; and being counterpois'd with a weight of Lead, was convey'd into the Receiver, and clos'd up in it. The Beam appearing to continue Horizontal, the Pump was set awork, and there scarce past above two or three Exsuctions of the Air, before the Ballance lost its *Æquilibrium*, and began to incline to that side on which the Bubble was; which, as the Air was further and further drawn out, did manifestly more and more preponderate, till he that pump'd began to grow weary of

his Imployment: after which the aire being leasurely let in againe, the scales by degrees returned to their former *Æquilibrium*. After that we tooke them out, and casting into that scale to which the lead belong'd three quarters of a grain, we convey'd the ballance into the Receiver, which being closed up, and exhausted as before, we observ'd, that as the aire was drawne out more and more, so the glasse bubble came neerer and neerer to an *Æquilibrium* with the other weight, till at length the beame was drawne to hang horizontall; which (as we had found by another tryall) wee could not bring it to do, when a quarter of a Graine more was added to the scale, to which the lead belong'd: though it seem'd questionlesse, that if wee could have perfectly empty'd the Receiver of the contain'd aire, that included in the bubble would have weighed above a grain, notwithstanding its having been probably somewhat Rarify'd by the flame by the help of which, the bubble was seald up. Let us adde, that on the regresse of the excluded air, the Lead, and the weight cast into the same

ſame ſcale, did againe very much preponderate.

We likewiſe convey'd into the Receiver, the ſame bubble, open'd at the end of the ſlender pipe above mentioned, but having drawne out the aire, after the accuſtomed manner, we found not as before, the bubble to out-weigh the oppoſite lead, ſo that by the help of our Engine, we can weigh the Aire, as we weigh other Bodies, in its naturall or ordinary conſiſtence, without at all condenſing it: Nay, which is remarkable, having convey'd a Lamb's bladder about halfe full of Aire into the Receiver, wee obſerved, that though upon the drawing out of the ambient aire the impriſoned Air ſo expanded it ſelf, as to diſtend the Bladder ſo, as to ſeem ready to break it; yet this rarified Air did manifeſtly depreſs the Scale whereunto it was annexed.

Another thing, we muſt not forget to mētion, that happend to us, whil'ſt we were making tryals cōcerning the weight of the Air; namely, That having once cauſ'd the

Pump

Pump to be ſomewhat obſtinately ply'd, to diſcover the better what may be expected from the thinneſs of the medium in this Experiment; the Impriſon'd Air broke its brittle Priſon, and throwing the greateſt part of it againſt the ſide of the Receiver, daſh'd it againſt that thick Glaſs into a multitude of pieces. Which Accident I mention, partly that it may confirm what we deliver'd in our Reflections, upon the firſt Experiment, where we conſider'd what would probably be done by the Spring of the Air Impriſon'd in ſuch Glaſſes, in caſe the ballancing preſſure of the ambient Air were withdrawn; and partly, that we may thence diſcern of how cloſe a Texture Glaſs is, ſince ſo very thin a film of Glaſs (if I may ſo call it) prov'd ſo impervious to the Air, that it could not get away through the Pores, but was forc'd to break the glaſs in pieces to free it ſelf; and this, notwithſtanding the time and advantage it had to try to get out at the Pores. And this I mention, that neither our Experiments, nor thoſe of divers Learned Men, might receive any prejudice from an Experiment which I happen'd to make divers years ago, and, which having been ſo much taken notice

of

of by curious Men, may be drawn to countenance their erroneous Opinion, who would fain perswade us, That Glass is penetrable by Air properly so called. Our Experiment was briefly this: We were distilling a certain substance, that much abounded with subtle Spirits and volatile Salt, in a strong Earthen-vessel of an unusual shape, to which was luted a large Receiver, made of the courser sort of Glass, (which the Trades-men are wont to call Green-glass) but in our absence, the Fire, though it were to be very strong, was by the negligence or mistake of those we appointed to attend it, so excessively increas'd, that when we came back to the Fornace we found the Spirituous and Saline Corpuscles pour'd out (if I may so call it) so hot, and so copiously into the Receiver, that they made it all opacous, and more likely to flie in pieces, then fit to be touch'd. Yet, being curious to observe the effects of a Distillation, prosecuted with so intense and unusual degree of heat, we ventur'd to come near, and observ'd, among other things, that on the out-side of the Receiver, at a great distance from the juncture, there was setled a round whitish Spot or two, which at

first we thought might be some stain upon the Glass; but after, finding it to be in divers Qualities like the Oyl, and Salt of the Concrete we were Distilling, we began to suspect that the most subtle and fugitive parts of the impetuously ascending Steams, had penetrated the substance (as they speak) of the Glass, and by the cold of the ambient Air were condens'd on the surface of it. And though we were very backward to credit this suspition, and therefore call'd in an Ingenious Person or two, both to assist us in the Observation, and have Witness of its event, we continued a while longer to watch the escape of such unctuous Fumes, and upon the whole matter unanimously concluded, That all things consider'd, the subtle parts of the distill'd matter being violently agitated, by the excessive heat had pass'd through the Pores of the Glass, widen'd by the same heat. But this having never happen'd but once in any of the Distillations we have either made or seen, though these be not a few, it is much more reasonable to suppose, that the perviousness of our Receiver to a Body much more
subtle

ſubtle then Air, proceeded partly from the looſer Texture of that particular parcel of Glaſs the Receiver was made of (for Experience has taught us, that all Glaſs is not of the ſame compactneſs and ſolidity) and partly from the enormous heat, which, together with the vehement agitation of the penetrant Spirits, open'd the Pores of the Glaſs; then to imagine that ſuch a ſubſtance as Air, ſhould be able to permeate the Body of Glaſs contrary to the teſtimony of a thouſand Chymical and Mechanical Experiments, and of many of thoſe made in our Engine, eſpecially that newly recited: Nay, by our fifth Experiment it appears, that a thin Bladder will not at its Pores give paſſage even to rarified Air. And on this occaſion we will annex an Experiment, which has made ſome of thoſe we have acquainted with it, doubt, whether the Corpuſcles of the Air be not leſſe ſubtle then thoſe of Water.

But without examining here the reaſonableneſſe of that doubt, we will proceed to recite the Experiment it ſelf, which ſeems to teach, That though Air,

when sufficiently compress'd, may perchance get entrance into narrower holes and crannies then Water; yet unless the Air be forc'd in at such very little holes, it will not get in at them, though they may be big enough to let Water pass through them.

The Experiment then was this: I took a fair Glass *Siphon*, the lower end of whose longest Leg was drawn by degrees to such a slenderness, that the Orifice, at which the Water was to fall out, would hardly admit a very small Pin: This *Siphon* being inverted, the matter was so order'd, that a little Bubble of Air was intercepted in the slenderest part of the *Siphon*, betwixt the little hole newly mention'd, and the incumbent Water, upon which, it came to pass, that the Air being not to be forc'd through so narrow a passage, by so light a Cylinder of Water, though amounting to the length of divers Inches, as lean'd upon it, hinder'd the further Efflux of the Water, as long as I pleas'd to let it stay in that narrow place: whereas, when by blowing a little at the wider end of the *Siphon*, that little parcel of Air was forc'd out with some Water, the remaining Water, that before

continu'd ſuſpended, began freely to drop down again as formerly. And if you take a Glaſs Pipe, whether it be in the form of a *Siphon*, or no, that being for the moſt part of the thickneſs of a Mans Finger, is yet towards one end ſo ſlender, as to terminate in a hole almoſt as ſmall as a Horſe-hair; and if you fill this Pipe with Water, you will finde that Liquor to drop down freely enough thorow the ſlender Extream: But if you then invert the Pipe, you will finde that the Air will not eaſily get in at the ſame hole through which the Water paſſ'd. For in the ſharp end of the Pipe, ſome Inches of Water will remain ſuſpended, which 'tis probable would not happen, if the Air could get in to ſucceed it, ſince if the hole were a little wider, the Water would immediatly ſubſide. And though it be true, that if the Pipe be of the length of many Inches, a great part of the Water will run down at the wider Orifice, yet that ſeems to happen for ſome other reaſon, then becauſe the Air ſucceeds it at the upper and narrow Orifice, ſince all the ſlender part of the Pipe, and perhaps ſome Inches more, will continue full of Water.

And

And on this occaſion I remember, that whereas it appears by our fifth Experiment, That the Aërial Corpuſcles (except perhaps ſome that are extraordinarily fine) will not paſſe thorow the Pores of a Lambs Bladder, yet Particles of Water will, as we have long ſince obſerv'd, and as may be eaſily try'd, by very cloſely tying a little *Alcalizate* Salt (we uſ'd the Calx of Tartar, made with Nitre) in a fine Bladder, and dipping the lower end of the Bladder in Water; for if you hold it there for a competent while, you will finde that there will ſtrain thorow the Pores of the Bladder Water enough to diſſolve the Salt into a Liquor.

But I ſee I am ſlipt into a Digreſſion, wherefore I will not examine, whether, the Experiment I have related, proceeded from hence, That the ſpringy Texture of the Corpuſcles of the Air, makes them leſs apt to yield and accommodate themſelves eaſily to the narrow Pores of Bodies, then the more flexible Particles of Water; or whether it may more probabiy be aſcrib'd to ſome other Cauſe. Nor will I ſtay to conſider how far we may hence be aſſiſted to gheſs at the cauſe of the aſcenſion of Water in the ſlender

Pipes and *Siphons* formerly mention'd, but will return to our Bubble; and take notice, That we thought fit also to endeavor to measure the capacity of the Bubble we had made use of, by filling it with Water, that we might the better know how much Water answered in weight to $\frac{3}{4}$ of a Grain of Air; but notwithstanding all the diligence that was used to preserve so brittle a Vessel, it broke before we could perfect what we were about, and we were not then provided of another Bubble fit for our turn.

The haste I was in, My Lord, when I sent away the last Sheet, made me forget to take notice to you of a Problem that occurr'd to my thoughts, upon the occasion of the slow breaking of the Glass Bubble in our evacuated Receiver. For it may seem strange, since by our sixth Experiment it appears, that the Air, when permitted, will by its own internal Spring expand it selfe twice as much as *Mersennus* was able to expand it, by the heat even of a candent *Æolipile*: Yet the *Elater* of the Air was scarce able to break a very thin Glass Bubble, and ut-

terly unable to break one ſomewhat thicker, within whoſe cavity it was impriſon'd; whereas Air pent up and agitated by heat is able to perform ſo much more conſiderable effects, that (not to mention thoſe of Rarefaction that are more obvious) the Learned Jeſuit *Cabæus* (he that writ of the Load-ſtone) relates, That he ſaw a Marble Pillar (ſo vaſt, that three men together with diſplay'd arms could not imbrace it, and that 1000 Yoke of Oxen drawing it ſeveral ways with all their ſtrength, could not have torn it aſſunder) quite broken off in the midſt, by reaſon of ſome Wood, which happening to be burnt juſt by the Pillar, the heat proceeding from the neighboring Fire, ſo rarified ſome Air or Spirituous Matter which was ſhut up in the cavities of the Marble, that it broke through the ſolid Body of the Stone to obtain room to expand it ſelf.

T: Nicol: Cab: lib: 4. Meteor: Ariſtot:

I remember I have taken notice that probably the reaſon why the included Air did not break the hermetically ſeal'd Bubbles that remain'd intire in our emptyed Receiver, was, That the Air, being ſomewhat rarefied by the Flame imploy'd to cloſe the Glaſs, its Spring, upon the re-

ceſs

cess of the heat, grew weaker then before. But though we reject not that ghess, yet it will not in the present case serve the turn, because that much smaller Glass bubbles exactly clos'd, will, by the included Air (though agitated but by the heat of a very moderate Fire) be made to fly in pieces. Whether we may be assisted to salve this Problem, by considering that the heat does from within vehemently agitate the Corpuscles of the Air, and adde its assistance to the Spring they had before, I shall not now examine: since I here but propose a Problem, and that chiefly that by this memorable Story of *Cabaus*, notice may be taken of the prodigious power of Rarefaction, which hereby appears capable of performing stranger things then any of our Experiments have hitherto ascrib'd to it.

We should hence, My Lord, immediatly proceed to the next Experiment, but that we think it fit, on this occasion, to acquaint You with what some former tryals (though not made in our Engine) have taught us, concerning what we would have discover'd by the newly mention'd Bubble that broke. And this the rather, because (a great part of this

letter supposing the gravity of the Aire) it will not be impertinent to determine more particularly then hitherto we have done, what gravity we ascribe to it.

We tooke then an *Æolipile* made of copper, weighing six ounces, five drachms, and eight and forty graines: this being made as hot as we durst make it, (for feare of melting the mettle, or at least the Sodar) was removed from the fire and immediately stopped with hard wax that no Aire at all might get in at the little hole wont to be left in *Æolipiles* for the fumes to issue out at: Then the *Æolipile* being suffer'd leasurely to coole was again weighed together with the wax that stopt it, and was found to weigh (by reason of the additionall weight of the wax) six ounces, sixe drachmes, and 39 graines. Lastly, the wax being perforated without taking any of it out of the Scale, the externall Aire was suffered to rush in (which it did with some noyse) and then the *Æolipile* and wax, being againe weighed amounted to six ounces, six drachmes, and 50. graines. So that the *Æolipile* freed as farre as our fire could free it, from it's Aire, weighed lesse then

it

it ſelfe when repleniſhed with Air, full eleven graines. That is, the Air containable within the cavity of the *Æolipile* amounted to eleven graines and ſomewhat more; I ſay ſomewhat more, becauſe of the particles of the Air, that were not driven by the fire out of the *Æolipile.* And by the way (if there be no miſtake in the obſervations of the diligent *Merſennus*) it may ſeeme ſtrange that it ſhould ſo much differ from 2. or 3. of ours; in none of which we could rarifie the Air in our *Æolipile* (though made red hot almoſt all over, and ſo immediately plung'd into cold water) to halfe that degree which he mentions, namely to 70. times it's naturall extent, unleſſe it were that the *Æolipile* he imploy'd was able to ſuſtaine a more vehement heat then ours (which yet we kept in ſo great an one, that once the ſoder melting, it fell aſunder into the two Hemiſpheres it conſiſts of.)

The fore-mentioned way of weighing the Air by the help of an *Æolipile*, ſeems ſomewhat more exact then that which *Merſennus* uſed, In that in ours the *Æolipile* was not weighed, till it was cold; whereas in his, being weighed red hot, it

ſubject to looſe of it's ſubſtance in the cooling, for (as we have elſewhere noted on another occaſion) Copper heated red hot is wont in the cooling to throw off little thin ſcales in ſuch plenty, that having purpoſely watcht a Copper *Æolipile* during its refrigeration, we have ſeen the place round about it almoſt covered with thoſe little ſcales it had every way ſcatter'd : which, however they amount not to much, ought not to be over-looked, when 'tis ſo light a body as Air, that is to be weighed. We will not examine, whether the *Æolipile* in cooling may not receive ſome little increment of weight, either from the vapid or ſaline Steames that wander up and downe in the Air: But we will rather mention, that (for the greater exactneſſe) we imployed to weigh our *Æolipile*, both when fill'd onely with Air and when repleniſht with Water, a paire of ſcales that would turne (as they ſpeak) with the fourth part of a grain.

As to the proportion of weight betwixt Air and Water, ſome learned men have attempted it by wayes ſo unaccurate that they ſeeme to have much miſtaken it. For (not to mention the improbable accounts of *Kepler* and others.) The learned

ned and diligent *Ricciolus*, having purposely endeavoured to investigate this proportion by meanes of a thin bladder, estimates the weight of the Air to that of the Water to be as one to ten thousand, or thereabouts. And indeed I remember that having formerly, on a certain occasion, weighed a large bladder full of Air, and found it when the Air was all squeesed out, to have contained fourteen graines of Air. I found the same bladder afterwards fill'd with water to containe very neer 14. pound of that liquor: according to which account, the proportion of Air to Water was almost as a graine to a pound, that is, as one to above 7600. To this we may adde, that on the other side, *Galileo* himselfe using another, but an unaccurate way too, defined the Air to be in weight to Water, but as one to 4 hundred. But the way formerly proposed of weighing the Air by an *Æolipile*, seemes by great oddes more exact; and (as farre as we could ghesse) seemed to agree well enough with the experiment made in our Receiver. Wherefore it will be best to trust our *Æolipile* in the enquiry we are about, and according to our observations the water it contained amounting to one

and twenty ounces and an halfe, and as much Air as was requisite to fill it weighing eleven graines, the proportion in gravity of Air to Water of the same bulk will be as one to 938. And though we could not fill the *Æolipile* with water, so exactly as we would, yet in regard we could not either as perfectly as we would, drive the Air out of it by heat; we think the proportion may well enough hold: but those that are delighted with round numbers (as the phrase is) will not be much mistaken if they reckon water to be neere a thousand times heavier than Air. And (for further proof that we have made the proportion betwixt these two bodies rather greater then lesser then indeed it is; and also to confirme our former observation of the weight of the Air) we will adde, That, having another time put some Water into the *Æolipile* before we set it on the fire, that the copious vapours of the rarefied liquor might the better drive out the Air, we found, upon tryall carefully made, that when the *Æolipile* was refrigerated, and the included vapours were by the cold turned againe into water (which could not have happen'd to the Air, that the preceeding Steams expell'd) the Air,

when

when it was let in, increaſ'd the weight of the *Æolipile* as much as before, namely, Eleven Grains; though there were already in it twelve Drachmes and a half, beſides a couple of Grains of Water, which remain'd of that we had formerly put into it to drive out the Air.

Merſennus indeed tells us, that by his account Air is in weight to Water, as 1 to 1356. And adds, that we may, without any danger, believe that the gravity of Water to that of Air of a like bulk, is not leſs then of 1300 to 1. And conſequently, that the quantity of Air to a quantity of Water equiponderant thereto, is as 1300 to 1. But why we ſhould relinquiſh our own carefully repeated tryals, I ſee not. Yet I am unwilling to reject thoſe of ſo accurate and uſeful a Writer: And therefore ſhall propoſe a way of reconciling our differing Obſervations, by preſenting, that the diſcrepance between them may probably ariſe from the differing conſiſtence of the Air at *London* and at *Paris*: For our Air being more cold and moiſt, then that which Your Lordſhip now breaths, may be ſuppoſ'd alſo to be a fourth or fifth part more heavy. I leave it to be conſider'd, whether it be of

any moment that our Obſervations were made in the midſt of Winter, whereas his were perhaps made in ſome warmer time of the Year. But I think it were not amiſs that, by the method formerly propos'd, the gravity of the Air were obſerv'd both in ſeveral Countries, and in the ſame Country, in the ſeveral Seaſons of the Year and differing Temperatures of the Weather. And I would give ſomething of value to know the weight of ſuch an *Æolipile* as ours full of air in the midſt of Winter in *Nova Zembla*, if that be true which we formerly took notice of, namely, That the *Hollanders*, who Wintered there, found that Air ſo thick that their Clock would not go.

If Your Lordſhip ſhould now ask me, if I could not by the help of theſe, and our other Obſervations, decide the Controverſies of our Modern Mathematicians about the height of the Air or Atmoſphere, by determining how high it doth indeed reach: I ſhould anſwer, That though it ſeems eaſie enough to ſhew that divers Famous and Applauded Writers have been miſtaken in aſſigning the heigth of the Atmoſphere: Yet it ſeems very difficult preciſely to define of what height

it is. And because we have hitherto but lightly touch'd upon a matter of such importance, we presume it wil not be thought impertinent, upon this occasion, to annex something towards the Elucidation of it.

What we have already try'd and newly set down, allows us to take it for granted, that (at least about *London*) the proportion of gravity betwixt Water and Air, of equal bulk, is as of a thousand to one.

The next thing therefore that we are to enquire after, in order to our present design, is the difference in weight betwixt Water and Quick-silver: And though this hath been defin'd already by the Illustrious *Verulam*, and some other inquisitive Persons, that have compar'd the weight of several Bodies, and cast their Observations into Tables, yet we shall not scruple to annex our own tryals about it: Partly, because we finde Authors considerably to dis-agree; partly, because we us'd exacter Scales, and a somewhat more wary method then others seem to have done: And partly also, because having prosecuted our inquiry by two or three several ways; the small difference be-

between the events may assure us that we were not much mistaken.

We took then a Glass Pipe, of the form of an inverted *Siphon*, whose shape is delineated in the sixteenth Figure: And pouring into it a quantity of Quick silver, we held it so, that the superficies of the Liquor, both in the longer and shorter leg, lay in a Horizontal Line, denoted in the Scheme by the prick'd Line E F; then pouring Water into the longer Leg of the *Siphon*, till that was almost fill'd, we observ'd the surface of the Quick-silver in that leg to be, by the weight of the Water, depress'd, as from E to B; and in the shorter leg, to be as much impell'd upward as from F to G: Whereupon having formerly stuck marks, as well at the point B, as at the opposite point D, we measur'd both the distance D C to have the height of the Cylinder of Quick-silver, which was rais'd above the Point D (level with the surface of the Quick-silver in the other leg) by the weight of the Water, and the distance B A which gave us the height of the Cylinder of Water. So that the distance D C amounting to $2\frac{11}{50}$ Inches, and the height of the Water amounting $30\frac{45}{57}$ Inches; and the whole num-

numbers on both sides, which the annex-ed Fractions being reduc'd to improper Fractions of the same denomination, the proportion appear'd to be (the denominators beng left out as equal on both sides) as 121 to 1665; or by reduction, as one to $13\frac{92}{121}$.

Besides this unusual way of determining the gravity of some things, we measur'd the proportion betwixt Quick-silver and Water, by the help of so exact a ballance, as looses its *Æquilibrium* by the hundredth part of a Grain. But because there is wont to be committed an over-sight in weighing Quick-silver and Water, especially if the Orifice of the Vessel wherein they are put be any thing wide, in regard that men heed not that the surface of Water in Vessels will be concave, but that of Quick-silver, notably convex or protuberant: To avoid this usual over-sight (I say) we made use of a glass bubble, blown very thin at the Flame of a Lamp, that it might not be too heavy for the Ballance, and terminating in a very slender neck, wherein the concavity or convexity of a Liquor could not be considerable: This Glass weighing $23\frac{1}{2}$ Grains, we fill'd almost

almoſt with Quick-ſilver, and faſtning a mark over againſt the middle of the protuberant Superficies as near as our Eyes could judge, we found that the Quick-ſilver alone weighed $299\frac{7}{32}$ Grains: Then the Quick-ſilver being pour'd out, and the ſame Glaſs being fill'd as full of common Water, we found the Liquor to weigh $21\frac{7}{8}$ Grains. Whereby it appear'd that the weight of Water to Quick-ſilver, is as one to $13\frac{19}{28}$: Though our Illuſtrious *Verulam* (queſtionleſs not for want of Judgement or Care, but of exact Inſtruments) makes the proportion betwixt thoſe two Liquors to be greater then of 1 to 17. And to adde, that upon the by, ſince Quick-ſilver and well rectified Spirit of Wine, are (how juſtly I ſay not) accounted, the one the heavieſt, and the other the lighteſt of Liquors; we thought to fill in the ſame Glaſs, and with the ſame Scales to obſerve the difference betwixt them, which we found to be as of 1 to $16\frac{641}{1084}$; whereby it appear'd, That the difference betwixt Spirit of Wine, that may be made to burn all away, (ſuch as was ours) and common Water, is as betwixt 1 and $1\frac{44}{171}$

We

We might here take occasion to admire, that though Water (as appear'd by the Experiment formerly mention'd of the Pewter Vessel) seems not capable of any considerable condensation, and seems not to have interspers'd in it any store of Air; yet Quick-silver, of no greater bulk then Water, should weigh near fourteen times as much. But having onely pointed at this as a thing worthy of consideration, we will proceed in our inquiry after the heigth of the Atmosphere: And to avoid the trouble of Fractions, we will assume that Quick-silver is fourteen times as heavy as Water, since it wants so little of being so.

Wherefore having now given us the proportion of Air to Water, and Water to Quick-silver, it will be very easie to finde the proportion betwixt Air and Quick-silver, in case we will suppose the Atmosphere to be uniformly of such a consistence as the Air we weighed here below. For since our Engine hath sufficiently manifested that 'tis the *Æquilibrium* with the external Air, that in the *Torricellian* Experiment keeps the Quick-silver from subsiding; And since, by our accurate Experiment formerly mention'd,

it

it appears that a Cylinder of Mercury, able to ballance a Cylinder of the whole Atmosphere, amounted to near about thirty Inches; and since, consequently we may assume the proportion of Quick-silver to Air to be as fourteen thousand to one; it will follow, that a Cylinder of Air, capable to maintain an *Æquilibrium*, with a Mercurial Cylinder of two Foot and an half in height, must amount to 35000 Feet of our English Measure; and consequently (reckoning five Foot to a Geometrical Pace, and one thousand such Paces to a Mile) to seven full Miles.

But this (as we lately intimated) proceeds upon the supposition, that the Air is every where of the same consistence that we found it near the surface of the Earth; but that cannot with any safety be concluded, not onely for the reason I finde to have been taken notice of by the Antients, and thus exprest in *Seneca*: *Omnis Aër* (says he) *quo propior est terris hoc crassior; quemadmodum in aqua & in omni humore fæx ima est, ita in Aëre spississima quæq; desidunt*; but much more, because the springy Texture of the Aërial Corpuscles, makes them capable of a

Senec: Nat: quest: lib. 4. cap. 10.

very

very great compression, which the weight of the incumbent part of the Atmosphere is very sufficient to give those that be undermost and near the surface of the Earth. And if we recall to minde those former Experiments, whereby we have manifested, That Air, much rarefied without heat, may easily admit a further rarefaction from heat; and that the Air, even without being expanded by heat, is capable of being rarefied to above one hundred and fifty times the extent it usually possesses here below; How can it be demonstrated that the Atmosphere may not, for ought we know, or at least for ought can be determin'd by our Statical and Mechanical Experiments, rise to the height of Five and twenty *German* Leagues, if not of some hundred of common Miles?

And this conjecture it self may appear very injurious to the height whereunto Exhalations may ascend, if we will allow that there was no mistake in that strange Observation made at *Tolous* in a clear Night in *August*, by the diligent Mathematician *Emanuel Magnan*, and thus Recorded by *Ricciolus*, (for I have not at hand the Authors own Book) *Vidit* (says he) *ab hora undecima post meridiem usq; ad*

Ricciol: Alma: Nov: Tom: 2. lib. 10. sect. 6. prop. 50. Ex magnan: lib. 1. Perspectivæ horariæ prop: 38.

mediam noctem Lunâ infra horizontem positâ, nubeculam quandam lucidam prope Meridianum fere usque ad Zenith diffusam quæ consideratis omnibus non poterat nisi à sole illuminari; ideoque altior esse debuit tota umbra terræ. Addit (continues *Ricciolus*) *simile quid evenisse Michaeli Angelo Riccio apud Sabinos versanti nempe viro in Mathesi eruditissimo.*

Various Observations made at the feet, tops, and interjacent parts of high Mountains, might perchance somewhat assist us to make an estimate in what proportion, if in any certain one, the higher Air is thicker then the lower, and ghess at the dis-form consistence, as to laxity and compactness of the Air at several distances from us. And if the difficulties about the refractions of the Celestial Lights, were satisfactorily determin'd, that might also much conduce to the placing due limits to the Atmosphere (whose Dimensions those Observations about Refractions seem hitherto much to contract.) But for the present we dare not pronounce any thing peremptorily concerning the height of it, but leave it to further inquiry: contenting our selves to have manifested the mistake of divers

eminent

eminent Modern Writers, who will not allow the Atmoſphere to exceed above two or three Miles in height (as the Famous *Kepler* will not the *Aër refractivus*) and to have rendred a reaſon why in the mention we made in the Notes upon the firſt Experiment, touching the height of the Atmoſphere, we ſcrupled not to ſpeak of it, as if it might be many Miles high.

WE will now proceed to recite a *Phænomenon*, which, though made amongſt the firſt, we thought fit not to mention till after many others, that we might have the opportunity to obſerve as many Circumſtances of it as we could, and ſo preſent Your Lordſhip at once, moſt of what we at ſeveral times have taken notice of concerning ſo odde a *Phænomenon*. *Experiment 37.*

Our Engine had not been long finiſh'd, when, at the firſt leaſure we could ſteal from our occaſions to make tryal of it, we cauſ'd the Air to be pump'd out of the Receiver; and whil'ſt I was buſied in entertaining a Learned Friend that juſt then came to viſit me, an Ingenious By-

ſtander, thought he perceiv'd ſome new kind of Light in the Receiver, of which giving me haſtily notice, my Friend and I preſently obſerv'd, that when the Sucker was drawn down, immediately upon the turning of the Key, there appear'd a kinde of Light in the Receiver, almoſt like a faint flaſh of Lightening in the Day-time, and almoſt as ſuddenly did it appear and vaniſh. Having, not without ſome amazement, obſerv'd divers of theſe Apparitions of Light, we took notice that the Day was clear, the hour about ten in the Morning, that the onely Window in the Room fac'd the North; and alſo, that by interpoſing a Cloak, or any opacous Body between the Receiver and the Window, though the reſt of the Room were ſufficiently enlightned, yet the flaſhes did not appear as before, unleſs the opacous Body were remov'd. But not being able on all theſe Circumſtances to ground any firm Conjecture at the cauſe of this ſurpriſing *Phænomenon*, as ſoon as Night was come, we made the Room very dark; and plying the Pump, as in the Morning, we could not, though we often try'd, find, upon the turning of the Key, ſo much as the

leaſt

least glimmering of Light; whence we inferr'd, that the flash appearing in the Receiver, did not proceed from any new Light generated there, but from some reflections of the light of the Sun, or other Luminous Bodies plac'd without it; though whence that Reflection should proceed, it pos'd us to conjecture.

Wherefore the next Morning, hoping to inform our selves better, we went about to repeat the Experiment, but though we could as well as formerly exhaust the Receiver, though the place wherein we made the tryal was the very same; and though other Circumstances were resembling, yet we could not discover the least appearance of Light all that Day, nor on divers others on which tryal was again fruitlesly made; nor can we to this very time be sure a Day before hand that these Flashes will be to be seen in our great Receiver. Nay, having once found the Engine in a good humour (if I may so speak) to shew this trick, and sent notice of it to our Learned Friend Doctor *Wallis*, who express'd a great

desire to see this *Phænomenon*, though he were not then above a Bow-shoot off, and made haste to satisfie his Curiosity; yet by that time he was come, the thing he came for was no longer to be seen; so that having vainly endeavored to exhibit again the *Phænomenon* in his presence, I began to apprehend what he might think of me, when unexpectedly the Engine presented us a flash, and after that a second, and as many more, as suffic'd to satisfie him that we might very well confidently relate, that we have our selves seen this *Phænomenon*, though not confidently promise to shew it others.

And this unsuccessfulness whereto our Experiment is lyable, being such, that by all our watchfulness and tryals, we could never reduce it to any certain Rules or Observations; since in all constitutions of the Weather, times of the Day, *&c.* it will sometimes answer, and sometimes dis-appoint our Expectations; We are much discourag'd from venturing to frame an *Hypothesis* to give an account of it: which if the Experiment did constantly succeed, might the more hopefully be attempted; by the help of the following *Phænomena* laid together: some of them pro-

produc'd upon tryals purposely made to examine the validity of the conjectures, other tryals had suggested.

First then we observ'd, that the Apparition of Light may be made as well by Candle-light, as by Day-light; and in whatever position the Candle be held, in reference to the Receiver, as on this or that hand of it, above it, beneath it, or any other way, provided the Beams of Light be not hinder'd from falling upon the Vessel.

Next, we noted that the flash appears immediately upon the turning of the Key, to let the Air out of the Receiver into the empty'd Cylinder, in so much that I remember not that when at any time in our great Receiver, the Stop-cock was open'd before the Cylinder was exhausted (whereby it came to pass that the Air did rather descend, then rush into the Cylinder) the often mention'd flash appear'd to our eyes.

Yet, we further observ'd, that when instead of the great Receiver we made use of a small Glass, not containing above a pound and a half of Water, the *Phænomenon* might be exhibited though the Stop-cock were open, provided the

Sucker were drawn nimbly down.

We noted too, that when we began to empty the Receiver, the appearances of Light were much more conſpicuous then towards the latter end, when little Air at a time could paſs out of the Receiver.

We obſerv'd alſo, that when the Sucker had not been long before well Oyl'd, and inſtead of the great Receiver, the ſmaller Veſſel above-mention'd was employ'd; We obſerv'd, I ſay, that then, upon the opening of the Stop-cock, as the Air deſcended out of the Glaſs into the empty'd Cylinder, ſo at the ſame time there aſcended out of the Cylinder into the Veſſel a certain Steam, which ſeem'd to conſiſt of very little Bubbles, or other minute Corpuſcles thrown up from the Oyl, rarefied by the attrition it ſuffered in the Cylinder. For at the ſame time that theſe Steams aſcended into the Glaſs, ſome of the ſame kinde manifeſtly iſſued out like a little Pillar of Smoke at the Orifice of the Valve, when that was occaſionally open'd. And theſe Steams frequently enough preſenting themſelves to our view, we found, by expoſing the Glaſs to a clear Light, that they

they were wont to play up and down in it, and so by their whiteishness, to emulate in some measure the apparition of Light.

For we likewise sometimes found, by watchful observation, that when the Flash was great, not onely at the very instant the Receiver lost of its transparency, by appearing full of some kinde of whitish substance; but that for some short time after the sides of the Glass continued somewhat opacous, and seem'd to be darken'd, as if some whitish Steam adher'd to the inside of them.

He that would render a Reason of the *Phænomenon*, whereof all these are not all the Circumstances, must doe two things; whereof the one is difficult, and the other little less then impossible: For he must give an Account not onely whence the appearing whiteness proceeds, but wherefore that whiteness does sometimes appear and sometimes not.

For our part, we freely confesse our selves at a losse about rendering a Rea-

a Reason of the less difficult part of the Problem: And though Your Lordship should ev'n press us to declare what Conjecture it was, that the above-recited Circumstances suggested to us, we should propose the thoughts we then had, no otherwise then as bare Conjectures.

In case then our *Phænomenon* had constantly and uniformly appear'd, we should have suspected it to have been produc'd after some such manner as follows.

First, we observ'd that, though that which we saw in our Receiver seem'd to be some kinde of Light, yet it was indeed but a whiteness which did (as hath already been noted) opacate (as some speak) the inside of the Glass.

Next we consider'd, that our common Air abounds with Particles or little Bodies, capable to reflect the Beams of Light. Of this we might easily give divers proofs, but we shall name but two: The one, that vulgar observation of the Motes that appear in Multitudes swimming up and down in the Air, when the Sun-beams shooting into a Room, or any other shady Place discover them, though otherwise the eye cannot distinguish them

from

from the rest of the Air: The other proof we will take from what we (and no doubt very many others) have observ'd, touching the Illumination of the Air in the Night. And we particularly remember, that, being at some distance from *London* one Night, that the People, upon a very well-come Occasion, testified their Joy by numerous Bon-fires; though, by reason of the Interposition of the Houses, we could not see the Fires themselves, yet we could plainly see the Air all enlighten'd over and near the City; which argu'd, that the lucid Beams shot upwards from the Fires, met in the Air with Corpuscles opacous enough to reflect them to our Eyes.

A third thing that we considered, was, That white may be produc'd (without excluding other ways, or denying invisible Pores in the solidest Bodies) when the continuity of a Diaphanous Body happens to be interrupted by a great number of Surfaces, which, like so many little Looking-glasses, do confusedly represent a multitude of little and seemingly contiguous Images of the elucid Body. We shall not insist on the explanation of this, but refer You for it to what we have

said

ſaid in another Paper (touching Colours.) But the Inſtances that ſeem to prove it are obvious: For Water or whites of Eggs beaten to froth, do loſe their tranſparency and appear white. And having out of one of our leſſer Receivers carefully drawn out the Air, and ſo order'd it, that the hole by which the Water was to get in, was exceeding ſmall, that the Liquor might be the more broken in its paſſage thorow it, we obſerv'd with pleaſure, That, the Neck being held under Water, and the little hole newly mention'd being open'd, the Water that ruſh'd in was ſo broken, and acquired ſuch a multitude of new Surfaces, that the Receiver ſeem'd to be full rather of Milk then Water. We have likewiſe found out, That by heating a lump of Cryſtal to a certain degree, and quenching it in fair Water, it would be diſcontinu'd by ſuch a multitude of Cracks, (which created new Surfaces within it) that though it would not fall aſunder, but retain its former ſhape, yet it would loſe its tranſparency, and appear white.

Upon theſe Conſiderations, My Lord, and ſome others, it ſeem'd not abſur'd to imagine, That upon the ruſhing of the Air

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out of the Receiver into the empty'd Cylinder, the Air in the Receiver being suddenly and vehemently expanded, the Texture of it was as suddenly alter'd, and the parts made so to shift places (and perhaps some of them to change postures) as during their new and vehement Motion and their varied Seituation, to disturb the wonted continuity and so the Diaphaneity of the Air; which (as we have already noted) upon its ceasing to be a transparent Body, without the interposition of colour'd things, must easily degenerate into white.

Several things there were that made this Conjecture seem the less improbable. As first, That the whiteness always appear'd greater when the Exsuction began to be made, whil'st there was store of Air in the Receiver, then when the Air was in great part drawn out. And next, That, having exhausted the Receiver, and apply'd to the hole in the Stop-cock a large bubble of clear Glass, in such a manner, that we could at pleasure let the Air pass out at the small Glass into the great one, and easily fill the small one with Air again, We observ'd with pleasure,

That

That upon the opening the paſſage betwixt the two Glaſſes, the Air in the ſmaller having ſo much room in the greater to receive it, the Diſſilition of that Air was ſo great, that the ſmall Viol ſeem'd to be full of Milk; and this Experiment we repeated ſeveral times. To which we may adde, That, having provided a ſmall Receiver, whoſe upper Orifice was ſo narrow that I could ſtop it with my Thumb, I obſerv'd, that when upon the Exſuction of the Air the capacity of the Glaſs appear'd white, if by a ſudden removal of my Thumb I let in the outward Air, that whiteneſs would immediately vaniſh. And whereas it may be objected, That in the Inſtance formerly mention'd, Water turning from perſpicuous to white, there intervenes the Air, which is a Body of a Heterogeneous nature, and muſt turn it into Bubbles to make it loſe its tranſparency. We may borrow an Anſwer from an Experiment we deliver in another Treatiſe, where we teach how to make two very volatile Liquors, which being gently put together are clear as Rock-water, and yet will almoſt in a moment, without the ſub-ingreſſion of Air to turn them into Bubbles, ſo

alter

alter the diſpoſition of their inſenſible parts, as to become a white and conſiſtent Body. And this happens not as in the precipitation of *Benjamin*, and ſome other Reſinous Bodies, which being diſſolv'd in Spirit of Wine, may, by the effuſion of fair Water, be turn'd into a ſeemingly Milky ſubſtance. For this whiteneſs belongs not to the whole Liquor, but to the Corpuſcles of the diſſolv'd Gum, which after a while ſubſiding leave the Liquor tranſparent, themſelves onely remaining white: Whereas in our caſe, 'tis from the vary'd texture of the whole formerly tranſparent fluid Body, and not from this or that part that this whiteneſſe reſults: For the Body is white thorowout, and will long continue ſo; and yet may, in proceſs of time, without any addition, be totally reduc'd into a tranſparent Body as before.

But beſides the Conjecture inſiſted on all this while, we grounded another upon the following Obſervation, which was, That having convey'd ſome ſmoke into our Receiver plac'd againſt a Window, we obſerv'd, that upon the exſuction of the Air, the Corpuſcles that were ſwimming in it, did manifeſtly enough make the Re-

ceiver

ceiver ſeem more opacous at the very moment of the ruſhing out of the Air: For conſidering that the whiteneſs, whoſe cauſe we enquire of, did but ſometimes appear, it ſeem'd not impoſſible but that at ſuch times the Air in the Receiver might abound with Particles, capable of reflecting the Light in the manner requiſite to exhibit a white colour, by their being put into a certain unuſual Motion. As may be in ſome meaſure illuſtrated by this, That the new motion of the freſhly mention'd Fumes, made the inſide of the Receiver appear ſomewhat darker then before: And partly by the nature of our formerly mention'd ſmoking Liquor, whoſe parts though they ſeem'd tranſparent whil'ſt they compoſ'd a Liquor, yet when the ſame Corpuſcles, upon the unſtopping of the Glaſs, were put into a new motion, and diſpoſ'd after a new manner, they did opacate that part of the Air they mov'd in, and exhibited a greater whiteneſs then that which ſometimes appears in our Pneumatical Veſſel. Nor ſhould we content our ſelves with this ſingle Inſtance, to manifeſt, That little Bodies, which being rang'd after one manner, are Diaphanous and Colourleſs, may, by being

being barely agitated, dispers'd, and consequently otherways rang'd, exhibite a colour, if we were not unwilling to rob our Collection of Experiments concerning Colours.

But, My Lord, I foresee You may make some Objections against our proposed ghess, which perhaps I shall scarce be able to answer, especially, if You insist upon having me render a Reason why our *Phænomenon* appears not constantly.

I might indeed answer, that probably it would do so, if instead of our great Receiver we use such a small Viol as we have lately mention'd, wherein the Dissilition of the Air being much greater, is like to be the more conspicuous: Since I remember not that we ever made our tryal with such small Vessels, without finding the expected whiteness to appear. But it would remain to be explicated, why in our great Receiver the *Phænomenon* should sometimes be seen, and oftentimes not appear. And though that Conjecture which we last made should not be rejected, yet if we were further press'd to assign a reason why the Air should abound with such Particles, as we there suppose, more at one

time then another, we are not yet provided of any better Anſwer, then this general one, That the Air about us, and much more that within the Receiver, may be much alter'd by ſuch cauſes as few are aware of: For, not to repeat thoſe probable Arguments of this Aſſertion which we have occaſionally mention'd here and there in the former part of this Epiſtle, we will here ſet down two or three Inſtances to verifie the ſame Propoſition. Firſt, I finde that the Learned *Joſephus Acoſta*, among other Judicious Obſervations he made in *America*, hath this concerning the Effects of ſome Winds; *There are* (ſays he) *Winds which naturally trouble the Water of the Sea, and make it green, and black; others, clear as Cryſtal.* Next, we have obſerv'd, That though we conveyd into the Receiver our Scales, and the *Pendula* formerly mention'd, clean and bright; yet after the Receiver had been empty'd, and the Air let in again, the gloſs or luſtre both of the one, and of the other, appear'd tarniſh'd by a beginning ruſt. And in the laſt place, we will ſubjoyn an Obſervation we made ſome Years ago, which hath been heard of by divers Ingenious Men, and ſeen by

Joſeph: Acoſta: Nat: & Mor: Hyſt: of the Indies, lib. 3. cap. 9.

by ſome of them: We had, with pure Spirit of Wine, drawn a Tincture out of a certain Concrete which uſes to be reckoned among Mineral Bodies; And this Tincture being very pure and tranſparent, we did, becauſe we put a great value upon it, put into a Cryſtal Viol which we carefully ſtopp'd, and lock'd up in a Preſs among ſome other things that we ſpecially priz'd. This Liquor being a Chymical Rarity, and beſides, very defecate and of a pleaſing Golden Colour; we had often occaſion to look upon it, and ſo to take notice, that one time it ſeem'd to be very much troubled, and not clear as it was wont to be: Whereupon we imagined, that though it would be ſomething ſtrange, yet it was not impoſſible that ſome Precipitation of the Mineral Corpuſcles was then happening, and that thence the Liquor was opacated; but, finding after ſome days that though the expected Precipitation had not been made, yet the Liquor, retaining its former vivid Colour, was grown clear again as before; we ſomewhat wondered at it, and locking it up again in the ſame Preſs, we reſolved to obſerve, both whether the like changes would again appear in

our Tincture; and whether in case they should appear, they would be ascribable to the alterations of the Weather. But though, during the greatest part of a Winter and a Spring, we took pleasure to observe, how the Liquor would often grow turbid, and after a while clear again: Yet we could not finde that these Mutations depended upon any that were manifest in the Air, whieh would be often dark and clouded, when the Tincture was clear and transparent; as on the other side, in clear Weather the Liquor would appear sometimes troubled, and more opacous. So that being unable to give an account of these odde changes in our Tincture (which we suppose we have not yet lost, though we know not whether it have lost its fickle Nature) either by those of the Air, or any thing else that occurr'd to our thoughts; we could not but suspect that there may be in divers Bodies, as it were Spontaneous Mutations, that is, such changes as depend not upon manifest Causes. But, My Lord, what has been all this while said concerning our *Phænomenon*, is offer'd to You, not as containing a satisfactory Account of it, but to assist You to give Your self one.

We

WE took a Glaſs Veſſel, open at the top, and into it we put a mixture of Snow and common Salt (ſuch a mixture as we have in another Treatiſe largely diſcourſed of) and into the midſt of this mixture we ſet a Glaſſe, of a Cylindrical form, cloſely ſtopp'd at the lower end with Plaiſter, and open at the upper, at which we fill'd it with common Water. Theſe things being let down into the Receiver, and the Pump being ſet awork, the Snow began to melt ſomewhat faſter then we expected; whether upon the account of the Exſuction of the Air, or becauſe there was but little of the Snow, or whether for any other Reaſon, it appeared doubtfull. But however, by that time the Receiver had been conſiderably exhauſted, which was done in leſſe then $\frac{1}{4}$ of an hour, we perceived the Water near the bottom of the Glaſs Cylinder to Freeze, and the Ice by a little longer ſtay, ſeem'd to encreaſe, and to riſe ſomewhat higher *Experiment 38.*

then the ſurface of the ſurrounding Liquor, whereinto almoſt all the Snow and Salt were reſolv'd. The Glaſs being taken out, it appear'd that the Ice was as thick as the inſide of the Glaſs it fill'd, though into that I could put my Thumb. The upper ſurface of the Ice was very concave, which whether it were due to any unheeded accident, or to the exſuction of the Air, we leave to be determin'd by further tryal. And laſtly, the Ice held againſt the Light, appear'd not deſtitute of Bubbles, though ſome *By-ſtanders* thought they were fewer then would have been found if the Water had been frozen in the open Air. The like Experiment we try'd alſo another time in one of our ſmall Receivers, with not unlike ſucceſs.

And on this occaſion, My Lord, give me leave to propoſe a Problem, which ſhall be this: Whence proceeds that ſtrange force that we may ſometimes obſerve in frozen Water, to break the Bodies that Impriſon it, though hard and ſolid? That there is ſuch a force in Water expos'd to Congelation, may be gather'd not onely from what may be often obſerv'd in Winter, of the burſting of

Glaſſes

Glaſſes too cloſe ſtopp'd, fill'd with Water or aqueous Liquors, but by Inſtances as much more conſiderable as leſs obvious. For I remember, that an Ingenious Stone-cutter not long ſince complain'd to me, That ſometimes, through the negligence of Servants, the Rain being ſuffered to ſoak into Marble Stones, the ſupervening violent Froſts would burſt the Stones, to the Profeſſors no ſmall damage. And I remember another Tradesman, in whoſe Houſe I had Lodgings, was laſt Winter complaining, that even Implements made of Bell-metal, being careleſly expoſ'd to the wet, have been broken and ſpoil'd by the Water, which, having gotten into the little Cavities and Crannies of the Metal, was there afterwards frozen and expanded into Ice. And to theſe Relations, we can adde one of the formerly mention'd *Cabæus's*, whereby they not onely may be confirm'd, but are ſurpaſſ'd: For he tells us, That he ſaw a huge Veſſel of exceeding hard Marble, ſplit aſunder by congel'd Water, whoſe Rarefaction, ſays our Author, prov'd ſo vehement, that the hardneſs of the Stone yielded to it; and ſo a Veſſel was broken, which would not have been ſo by 100 Yoke

In lib. 4. Meteor. Ariſt.

Yoke of Oxen drawing it ſeveral ways. I know, My Lord, that to ſolve this Problem, it will be ſaid, That Congelation does not(as is commonly, but erroneouſly preſum'd) reduce water into leſs room then it poſſeſſ'd before, but rather makes it take up more. And I have elſewhere prov'd by particular Experiments, That whether or no Ice may be truly ſaid to be Water rarefi'd (for that ſeems queſtionable) it may be ſaid to take up more room then the Water did before Glaciation. But though we grant that freezing makes Water ſwell, yet, how Cold (which in Weather-Glaſſes manifeſtly condences the Air) ſhould expand either the Water, or the intercepted Air ſo forcibly, as to perform ſuch things as we have newly related, will yet remain a Problem.

Experiment 39.

VVE took an Oval Glaſs, clear and (leaſt it ſhould break) pretty ſtrong, with a ſhort Neck at the obtuſer end; through this Neck, we thruſt almoſt to the bottom, a Pipe of Glaſs, which was cloſely Cemented to the newly mention'd Neck, the upper part of which Pipe, was drawn in ſome places more ſlender

then

then a Crows Quill, that the changes of the Air in that Glaſs Egge might be the more conſpicuous; Then there was convey'd into the Glaſs five or ſix Spoonfulls of Water, part of which, by blowing Air into the Egge, was raiſ'd into the above-mention'd ſlender part of the Pipe, ſo that the Water was interpoſ'd between the external Air, and that included in the Egge. This Weather-glaſs (delineated in the fourteenth Figure) was ſo plac'd, and cloſ'd up in the cavity of one of our ſmall Receivers, that onely the ſlender part of the Pipe, to the heigth of four or five Inches, paſſing thorow a hole in the Cover, remain'd expoſ'd to the open Air.

The Pump being ſet a work, upon the Exſuction of the Air, the Water in the Pipe deſcended about a quarter of an Inch, and this upon two or three reiterated tryals; which ſeem'd ſufficiently to argue that there was no heat produc'd in the Receiver upon the Exſuction of the Air: For even a little heat would probably have been diſcover'd by that Weather-glaſs, ſince upon the bare application of my hand to the outſide of the Receiver, the warmth having after ſome

time

time been communicated or propagated through both the Glasses, and the interval betwixt them to the Imprison'd Air, did so rarifie that, as to inable it, by pressing upon the subjacent Water, to impel that in the Pipe very many times as far as it had fallen downwards upon the Exsuction of the Air.

Yet shall not we conclude, that in the cavity of the Receiver the cold was greater after the Exsuction of the Air then before.

For if it be demanded what then could cause the fore-mention'd subsiding of the Water? it may be answered, That probably it was the reaching of the Glass Egge, which, upon the Exsuction of the ambient Air, was unable to resist altogether as much as formerly the pressure of the included Air, and of the Atmosphere, which, by the intervension of the Water, press'd upon its concave surface: Which seem'd probable, as well by what was above deliver'd, in the Experiment about the breaking of the Glass by the force of the Atmosphere; as by this notable Circumstance (which we divers times observ'd) That when by drawing the Air out of the Receiver, the Water in the Pipe

was ſubſided, upon the readmiſſion of the external Air to preſs againſt the convex ſurface of the Egge, the Water was preſently re-impell'd to its former height: Which would perhaps appear leſs ſtrange to Your Lordſhip, if You had yet ſeen what we have heretofore taught in another Treatiſe concerning the Spring that may be diſcover'd in Glaſs, as rigid and inflexible a Body as it is generally eſteem'd. And in the mean while it may ſerve the turn to cauſe a Glaſs Egge to be blown exceeding thin, and then, having broken it, try how far you can by degrees bend ſome narrow parts of it; and how readily, upon the removal of what kept it bent, it will reſtore it ſelf to its former ſtate or poſture. But to return to our Experiment, From thence it ſeems probable, either that there ſucceeds no Body in the room of the Air drawn out of our Receiver, or that it is not every Matter that is ſubtle enough readily to paſs through the Pores of Glaſs, that is always agitated enough to produce Heat where ever it is plentifully found. So that if no *Vacuum* be to be admitted, this Experiment ſeems to invite us to allow a great diſparity, either as to bulk, or as to

agita-

agitation, or as to both, betwixt some parts of the Etherial Substance, and those that are wont here below to produce Heat and Fire.

We try'd also what Operation the drawing out of the Air would have upon Camphire, that being a Body, which, though not a Liquor, consists of such Volatile or Fugitive parts, that without any greater agitation then that of the open Air it self, they will copiously flie away. But we found not that even this loose Body was sensibly alter'd by the Exsuction of the ambient Air.

Experiment 40. IT may seem well worth trying, whether or no in our exhausted Glass the want of an ambient Body, of the wonted thicness of Air, would disable even light and little Animals, as Bees, and other winged Insects, to flie. But though we easily foresaw how difficult it would be to make such an Experiment; yet not to omit our endeavors, we procur'd a large Flesh-fly, which we convey'd into a small Receiver. We also another time shut into a great Receiver a Humming Bee, that appear'd strong and lively, though we had rather

have

have made the tryal with a Butter-fly, if the cold Season would have permitted us to finde any. * The Fly, after some Exsuctions of the Air, dropp'd down from the side of the Glass whereon she was walking: But, that the Experiment with the Bee might be the more instructive, we convey'd in with her a bundle of Flowers, which remain'd suspended by a string near the upper part of the Receiver: And having provok'd the Bee, we excited her to flie up and down the capacity of the Vessel, till at length, as we desir'd, she lighted upon the Flowers; whereupon we presently began to draw out the Air, and observ'd, That though for some time the Bee seem'd to take no notice of it, yet within awhile after she did not flie, but fall down from the Flowers, without appearing to make any use of her Wings to help her self. But whether this fall of the Bee, and the other Insect, proceeded from the mediums being too thin for them to flie in, or barely from the weakness, and as it were swooning of the Animals themselves, you will easily gather from the following Experiment.

Since the writing of this XLth Experiment, we procur'd a white Butter-Fly, and inclos'd it in one of our smaller Receivers, where, though at first he fluttered up and down, yet presently, upon the exsuction of the Air, he fell down as in a swoon, retaining no other motion then some little trembling of the Wings.

To

Experi-ment 41.

TO ſatisfie our ſelves in ſome meaſure, about the account upon which Reſpiration is ſo neceſſary to the Animals, that Nature hath furniſh'd with Lungs, we took (being then unable to procure any other lively Bird, ſmall enough to be put into the Receiver) a Lark, one of whoſe Wings had been broken by a ſhot, of a Man that we had ſent to provide us ſome Birds for our Experiment; but notwithſtanding this hurt, the Lark was very lively, and did, being put into the Receiver, divers times ſpring up in it to a good height. The Veſſel being haſtily, but carefully cloſ'd, the Pump was diligently ply'd, and the Bird for a while appear'd lively enough; but upon a greater Exſuction of the Air, ſhe began manifeſtly to droop and appear ſick, and very ſoon after was taken with as violent and irregular Convulſions, as are wont to be obſerv'd in Poultry, when their heads are wrung off: For the Bird threw her ſelf over and over two or three times, and dyed with her Breaſt upward, her Head downwards, and her Neck awry. And though upon the appearing of theſe Con-

vulſions, we turn'd the Stop-cock, and let in the Air upon her, yet it came too late; whereupon, caſting our eyes upon one of thoſe accurate Dyals that go with a *Pendulum*, and were of late ingeniouſly invented by the Noble and Learned *Hugenius*, we found that the whole Tragedy had been concluded within ten Minutes of an hour, part of which time had been imploy'd in cementing the Cover to the Receiver. Soon after we got a Hen-ſparrow, which being caught with Bird-lime was not at all hurt; when we put her into the Receiver, almoſt to the top of which ſhe would briskly raiſe her ſelf, the Experiment being try'd with this Bird, as it was with the former, ſhe ſeem'd to be dead within ſeven minutes, one of which were imploy'd in cementing on the Cover: But upon the ſpeedy turning of the Key, the freſh Air flowing in, began ſlowly to revive her, ſo that after ſome pantings ſhe open'd her eyes, and regain'd her feet, and in about a $\frac{1}{4}$ of an hour, after threatned to make an eſcape at the top of the Glaſs, which had been unſtopp'd to let in the freſh Air upon her: But the Receiver being cloſ'd the ſecond time, ſhe

was

was kill'd with violent Convulsions; within five Minutes from the beginning of the Pumping.

A while after we put in a Mouse, newly taken, in such a Trap as had rather affrighted then hurt him; vvhil'st he vvas leaping up very high in the Receiver, vve fasten'd the Cover to it, expecting that an Animal used to live in narrow holes vvith very little fresh Air, vvould endure the vvant of it better then the lately mention'd Birds: But though, for a vvhile after the Pump vvas set avvork, he continued leaping up as before; yet 'tvvas not long ere he began to appear sick and giddy, and to stagger, after vvhich he fell dovvn as dead, but vvithout such violent Convulsions as the Birds died vvith. Whereupon, hastily turning the Key, we let in some fresh Air upon him, by vvhich he recovered, after a vvhile, his senses and his feet, but seem'd to continue vveak and sick: But at length, grovving able to skip as formerly, the Pump vvas plyed again for eight minutes, about the middle of vvhich space, if not before, a very little Air by a mischance got in at the Stop-cock; and about tvvo minutes after that, the Mouse divers times leap'd up

lively

lively enough, though after about two minutes more he fell down quite dead, yet with Convulsions far milder then those wherewith the two Birds expired. This alacrity so little before his death, and his not dying sooner then at the end of the eighth minute, seem'd ascribable to the Air (how little soever) that slipt into the Receiver. For the first time, those Convulsions (that, if they had not been suddenly remedied, had immediately dispatch'd him) seis'd on him in six minutes after the Pump began to be set awork. These Experiments seem'd the more strange, in regard, that during a great part of those few minutes the Engine could but considerably rarefie the Air (and that too, but by degrees) and at the end of them there remain'd in the Receiver no inconsiderable quantity; as may appear by what we have formerly said of our not being able to draw down Water in a Tube, within much less then a Foot of the bottom: with which we likewise consider'd, that by the exsuction of the Air and interspersed Vapors, there was left in the Receiver a space some hundreds of times exceeding the bigness of the Animal, to receive the fuliginous Steams, from which

expiration discharges the Lungs; and, which in the other cases hitherto known, may be suspected, for want of room, to stifle those Animals that are closely pent up in too narrow Receptacles.

I forgot to mention, that having caus'd these three Creatures to be open'd, I could, in such small Bodies, discover little of what we sought for, and what we might possibly have found in larger Animals; for though the Lungs of the Birds appear'd very red, and as it were inflam'd, yet that colour being usual enough in the Lungs of such winged Creatures, deserves not so much our notice, as it does, That in almost all the destructive Experiments made in our Engine, the Animals appear'd to die with violently Convulsive Motions: From which, whether Physicians can gather any thing towards the Discovery of the Nature of Convulsive Distempers, I leave to them to consider.

Having proceeded thus far, though (as we have partly intimated already) there appear'd not much cause to doubt, but that the death of the fore-mention'd Animals proceeded rather from the want of Air, then that the Air was over-clogg'd by the steams of their Bodies, exquisitely

ly pent up in the Glaſs; yet I, that love not to believe any thing upon Conjectures, when by a not over-difficult Experiment I can try whether it be True or no, thought it the ſafeſt way to obviate Objections, and remove Scruples, by ſhutting up another Mouſe as cloſe as I could in the Receiver, wherein it liv'd above three quarters of an hour; and might probably have done ſo much longer, had not a *Virtuoſo* of quality, who in the mean while chanc'd to make me a Viſit, deſir'd to ſee whether or no the Mouſe could be kill'd by the exſuction of the ambient Air, whereupon we thought fit to open, for a little while, an intercourſe betwixt the Air in the Receiver, and that without it, that the Mouſe might thereby (if it were needful for him) be refreſh'd, and yet we did this without uncementing the Cover at the top, that it might not be objected, that perhaps the Veſſel was more cloſely ſtopp'd for the exſuction of the Air then before.

The Experiment had this event, that after the Mouſe had liv'd ten Minutes, (which we aſcrib'd to this, that the Pump, for want of having been lately Oyl'd, could move but ſlowly, and could not by

him that manag'd it, be made to work as nimbly as it was wont) at the end of that time he dy'd with Convulsive Fits, wherein he made two or three bounds into the Air, before he fell down dead.

Nor was I content with this, but for Your Lordships further satisfaction, and my own, I caus'd a Mouse, that was very hungry, to be shut in all Night, with a Bed of Paper for him to rest upon: And to be sure that the Receiver was well clos'd, I caus'd some Air to be drawn out of it, whereby, perceiving that there was no sensible leak, I presently re-admitted the Air at the Stop-cock, lest the want of it should harm the little Animal; and then I caus'd the Engine to be kept all Night by the Fire side, to keep him from being destroy'd by the immoderate cold of the Frosty Night. And this care succeeded so well, that the next Morning I found that the Mouse not onely was alive, but had devour'd a good part of the Cheese that had been put in with him. And having thus kept him alive full twelve hours, or better, we did, by sucking out part of the Air, bring him to droop, and to appear swell'd; and by letting in the Air again, we soon reduc'd him to his former livelinese.

A Digreſſion containing ſome Doubts touching Reſpi- ration.

I Fear Your Lordſhip will now expect, that to theſe Experiments I ſhould add my Reflections on them, and attempt, by their aſſiſtance, to reſolve the Diffi- culties that occur about Reſpiration; ſince at the beginning I acknowledg'd a further Enquiry into the Nature of that, to have been my Deſign in the related Tryals. But I have yet, becauſe of the inconve- nient Seaſon of the Year, made ſo few Experiments, and have been ſo little ſa- tisfied by thoſe I have been able to make, that they have hitherto made Reſpiration appear to me rather a more, then a leſs Myſterious thing, then it did before. But yet, ſince they have furniſh'd me with ſome ſuch new Conſiderations, concern- ing the uſe of the Air, as confirms me in my Diffidence of the Truth of what is commonly believ'd touching that matter; That I may not appear ſullen or lazy, I am content not to decline employing a

few hours in ſetting down my Doubts, in preſenting Your Lordſhip ſome Hints, and in conſidering whether the Tryals made in our Engine, will at leaſt aſſiſt us to diſcover wherein the Deficiency lies that needs to be ſupply'd.

And this, My Lord, being all my preſent Deſign, I ſuppoſe You will not expect that (as if You knew not, or had forgotten what Anatomiſts are wont to teach) I ſhould entertain You with a needleſs Diſcourſe of the Organs of Reſpiration, and the variety of their Structure in ſeveral Animals; though if it were neceſſary, and had not been perform'd by others, I ſhould think, with *Galen*, that by treating of the Fabricks of living Bodies, I might compoſe Hymns to the wiſe Author of Nature, who, in the excellent contrivance of the Lungs, and other parts of (thoſe admirable Engines) Animals, manifeſts himſelf to be indeed what the Eloquent Prophet moſt juſtly ſpeaks him, *Wonderful in Councel, and excellent in working.*

Galenus de uſu, Part: lib: 3.

Iſa. 28. 29.

Nor ſhall we any further meddle with thoſe Controverſies ſo much agitated among the Moderns, namely, *Whether the motion of the Lungs in Reſpiration be their own,*

own, or but consequent to the motion of the Thorax, Diaphragme, and (as some Learned Men would have it) *the Abdomen; And, Whence it is that the Air swells the Lungs in Inspiration,* any further then they may receive light from our Engine: But that it may appear what kinde of service it is that may be expected from it on this occasion, we must premise a few Words to shew wherein the strength of the Objection we are to answer, lies: In favor then of those that would have the Lungs rather passive then active in the business of Respiration, it may against the common opinion be alledg'd, That as the Lungs being destitute of Muscles and of Fibres, are unfit to dilate themselves; so it appears, that without the motion of the *Thorax* they would not be fill'd with Air. Since as our Learned Friend Dr. *Highmore* has well (and congruously, to what our selves have purposely try'd) observ'd, if a live Dog have a great wound made in his Chest, the Lobes of the Lungs on that side of the *Mediastinum* will subside and lie still; the *Thorax* and the Lobes on the other side of the *Mediastinum*, continuing their former motion. And if suddenly at once the

the Muscles of the Chest be on both sides dissected, upon the Ingress of the Air, the whole Lungs, though untouch'd, will remain moveless, at least, as to any expansion or contraction of their substance.

To which we may adde the Observation of the diligent *Bartholinus*, who affirms the like of the *Diaphragme* also, namely, That it being wounded, the Lungs will fall together, and the Respiration cease, which my Experiments oppose not, provided the Wound be any thing great. And indeed the *Diaphragme* seems the principal Instrument of ordinary and gentle Respiration, although to restrain'd Respiration (if I may so call it) the intercostal Muscles, and perhaps some others may be allowed eminently to concur. But the chief of the Controversies formerly pointed at, is not yet decided, namely, what it is that conveys the Air into the Lungs. For when, to counterballance all that has been alledg'd, those that plead for the Lungs, demand what it is that should bring the Air into the Lungs, if themselves do not attract it, their Antagonists disagree about the Reply. For when to this question some of the best Modern Philosophers answer,

that

that by the dilatation of the Chest the contiguous Air is thrust away, and that pressing upon the next Air to it, and so onwards, the Propulsion is continued till the Air be driven into the Lungs, and so dilate them: When this (I say) is answered, it is Objected even by *Bartholine* himself, as a convincing Reply, that, according to this Doctrine, a Man could not fetch his Breath from a great Vessel full of Air, with a slender Neck, because, that when his Mouth covers the Orifice of the Neck, the dilatation of his *Thorax* could not propell the Air in the Vessel into his Lungs, by reason of its being separated by the inclosing Vessel from the ambient Air; and yet, say they, Experience witnesses that out of such a Vessel a Man may suck Air. But of this difficulty our Engine furnishes us with an easie Solution, since many of the former Experiments have manifested, That in the case proposed, there needs not be made any (though 'tis true that in ordinary Respiration there is wont to be made some) propulsion of the Air by the swelling *Thorax* or *Abdomen* into the Lungs; since upon the bare Dilatation of the *Thorax*, the Spring of that internal Air, or halituous substance that is wont

to

to possess as much of the Cavity of the Chest as the Lungs fill not up, being much weaken'd, the external and contiguous Air must necessarily press in at the open Winde-Pipe into the Lungs, as finding there less resistance then any where else about it.

And hence (by the way) we may derive a new assistance to judge of that famous Controversie disputed among Naturalists and Physitians, ever since *Galens* time, some maintaining that the Chest, with the contained Lungs, may be resembled to a pair of Bellows, which comes therefore to be fill'd because it was dilated: And others pleading to have the comparison made to a Bladder, which is therefore dilated because it is fill'd. For as to the *Thorax*, it seems evident from what has been lately said, that it, like a pair of Bellows, happens to be partly fill'd with Air, but because it was dilated: But as for the Lungs themselves, who want Fibres to distend them, they may fitly enough be compar'd to a Bladder; since they are dilated by being fill'd, namely, by that Air which rushes into them upon the dilatation of the Chest, in whose increased Cavity it findes (as we freshly noted) less resistance

ance to its Spring then elsewhere. And this brings into my minde that strange Observation of *Nicolaus Fontanus*, a Physitian at *Amsterdam*, who testifies, That in a Boy of the same Town, four years old, there was found, instead of Lungs, a certain Membranous Bladder; which being fill'd with Wind, and furnish'd with little Veins, had its origination from the Wind-Pipe it self; which being suppos'd true, how well it will agree with most of the Opinions touching Respiration, I leave to be considered.

Fontanus apud Bartholin: lib. 2. cap. 9.

And thus may the grand Objection of *Bartholine*, and others, be answered: But I leave to Anatomists to consider what is to be said to some Observations that seem to contradict those Anatomical Experiments already mention'd: Such was particularly that which I remember I have read in *Sennertus* (from the observation of his Father-in-law *Schato*) of a Melancholy Student, who having stabb'd himself, and pierced the *Diaphragme* in the thinner or tendonous part (call'd by many the Nervous Circle) lived seven Moneths after he had so wounded himself, though after his death (preceded by violent Vomitings)

ings) the Wound (perchance dilated by those strainings) appear'd so great, that the whole Stomack was found to have got in by it into the left side of the *Thorax*. And such also was the accident that happen'd to a Noble Man, whom I remember I have seen, and who is yet alive, in whose Chest there has, for these many years, remain'd a hole so great, that the motion of his Heart may be perceiv'd by it. These (I say) and some other Observations, I shall now forbear to insist on, because I hold it not unfit, before we come to consider the use of Respiration, that we acquaint Your Lordship with an Ingenious Conjecture, that was made at the cause of the hasty death of the Animals our Engine kill'd: namely, That it was not the want of Air that destroy'd them, but the Pressure of the innate Air in the cavity of the Chest; as if the Spring of this Air being no longer counterballanc'd by the ambient Air, was thereby become so strong, that it kept the *Thorax* forcibly distended, and hinder'd its wonted contraction; and so compress'd the Lungs and their Vessels, as to obstruct the Circulation of the Blood. And this

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Conjecture, as it is specious enough, so I might have admitted it for true; but that I consider'd, that (not to mention that one, especially of the Animals kill'd in our Engine, seem'd manifestly for a pretty while, and not long before he dy'd, to move his *Thorax*, as if he exercis'd Respiration) the diligent *Wallaus* relates, That he divers times observ'd, in the Dissection of live Bodies, that the Membrane that invests the Lungs, had Pores in it as big as the larger sort of Peas, which agrees with the Observations of Chyrurgions and Physitians, *viz.* That matter collected in the *Thorax*, has penetrated into the Lungs, and been discharged by coughing. And I remember too, that most of the Animals we kill'd in our Engine were Birds, of whose Lungs *Harvey* somewhere informs us, That he observ'd them very manifestly to open at their Extremities into the *Abdomen*. And by such Perforations we may well suppose the passage free betwixt the external Air and that in the *Abdomen*: But this Conjecture may be further consider'd. Besides; to show that the Animals that died in our Glasses, need not be suppos'd

to have been kill'd by the want of Air, we foresee another Argument that we must deal so ingeniously with Your Lordship, as not to conceal. You very well know, that besides the generality of the Schools, there are many new Philosophers who, though they dissent from the old Peripateticks in other things, do, as they, deny the possibility of a *Vacuum*; and hold, that those spaces which are devoid of Air, and other grosser Bodies, are all of them exactly replenished with a certain Etherial Matter, so thin and subtle, that it can freely permeate the Pores of the compactedst and closest Bodies, and ev'n of Glass it self. Now some of those Naturalists that are of this perswasion may object, That the Animals that died in our Receivers, did so, not so much for lack of Air, as by reason that the Air that was pump'd out was necessarily succeeded by an Etherial Substance; which consisting of parts vehemently agitated, and so very small, as without resistance to pass in and out through the very Pores of Glass; it may well be suppos'd, that a considerable quantity of this restless and subtle Matter, meeting together in the Receiver,

with

with the excessive heat of it, may be quickly able to destroy a little Animal, or at least, make the Air too intemperately hot to be fit for Respiration.

But though this be a Difficulty not so easily to be resolv'd without the assistance of our Engine, yet I suppose we have already answer'd the Objection by our 38th and 39th Experiments; which though we made partly for other purposes, yet we premis'd them onely to clear up the difficulty propos'd.

Another suspition we should have entertain'd concerning the death of our Animals, namely, That upon the sudden removal of the wonted pressure of the ambient Air, the warm Blood of those Animals was brought to an Effervescence or Ebullition, or at least so vehemently expanded, as to disturb the Circulation of the Blood, and so disorder the whole Oeconomy of the Body. (This (I say) I should have had some suspition of) but that Animals of a hot Constitution are not the sole ones that cannot in our exhausted Engine exercise the Function of Life. But I must not now dwell upon matters of this nature, because I think it high time to proceed to the considerati-

on

on of the principal subject of our Engine, namely, The use of Respiration; or rather, The use of the Air in Respiration. For whereas of the divers uses of it mention'd by Anatomists the most, such as the Production and Modulation of the Voice by the Elision of the Air, the *Larynx* &c. the expulsion of Excrements by Coughing, the conveying in of Odours by Inspiration, and some others, rather convenient for the well being of an Animal, then absolutely necessary to his Life: Whereas (I say) the other uses are such as we have said, The great *Hippocrates* himself gives this notable Testimony to the use of the Air, as to Animals endow'd with Lungs, *Mortalibus* (says he) *hic* (spiritus) *tum vitæ, tum morborum ægrotis causa est. Tantâque corporibus omnibus spiritûs inest necessitas, ut siquidem aliis omnibus & cibis & potionibus, quis abstineat, duos tamē aut tres, vel plures dies possit vitam ducere: At si quis spiritus in corpus vias intercipiat, vel exiguâ diei parte, homini pereundum sit; Adeo necessarius est usus spiritûs in corpore. Ad hæc quoq;, quum omnibus aliis actionibus homines quiescant, quod mutationibus innumeris vita sit exposita, ab hâc tamen solâ actione nun-*

quam

quam desistant animantia, quin aut spiritum adducant, aut reddant.

But touching the account upon which the Inspiration and Exspiration of Air (both which are comprehended in ἀναπνοὴ, Respiration) is so necessary to Life, both Naturalists and Physitians do so disagree, that it will be very difficult either to reconcile their Opinions, or determine their Controversies.

For first, Many there are who think the chief (if not sole) use of Respiration to be the Cooling and tempering of that Heat in the Heart and Blood, which otherwise would be immoderate: And this Opinion, not onely seems to be most received amongst Scholastick Writers, but divers of the new Philosophers, Cartesians, and others, admitted with some variation; teaching, That the Air is necessary, by its coldness, to condense the Blood that passes out of the right Ventricle of the Heart into the Lungs, that thereby it may obtain such a consistence, as is requisite to make it fit Fewel for the vital Fire or Flame, in the left Ventricle of the heart. And this Opinion seems favor'd by this, That Fishes, and other cold Creatures, whose Hearts have but one cavity, are al-

so unprovided of Lungs, and by some other considerations. But though it need not be deny'd, that the inspir'd Air may sometimes be of use by refrigerating the Heart; yet (against the Opinion that makes this Refrigeration, the most genuine and constant use of the Air) it may be Objected, That divers cold Creatures (some of which, as particularly Frogs, live in the Water) have yet need of Respiration, which seems not likely to be needed for Refrigeration by them that are destitute of any sensible heat, and besides, live in the cold Water: That even decrepid old Men, whose natural heat is made very languid, and almost extinguish'd by reason of age, have yet a necessity of frequent Respiration: That a temperate Air is fittest for the generality of breathing Creatures; and as an Air too hot, so also an Air too cold, may be inconvenient for them (especially, if they be troubled with an immoderate degree of the same Quality which is predominant in the Air:) That in some Diseases the natural heat is so weaken'd, that in case the use of Respiration were to cool, it would be more hurtful then beneficial to breath; and the suspending of the Respiration, may supply

ply the place of those very hot Medicines that are wont to be employ'd in such Distempers: That Nature might much better have given the Heart but a moderate heat, then such an excessive one, as needs to be perpetually cool'd, to keep it from growing destructive; which the gentle, and not the burning heat of an Animals Heart, seems not intense enough so indispensably to require. These, and other Objections, might be oppos'd, and press'd against the recited Opinion: But we shall not insist on them, but onely adde to them, That it appears not by our foregoing Experiments (I mean the 38th and 39th) that in our exhausted Receiver, where yet Animals die so suddenly for want of Respiration, the ambient Body is sensibly hotter then the common Air.

Other Learned Men there are, who will have the very substance of the Air to get in by the Vessels of the Lungs, to the left Ventricle of the Heart, not onely to temper its heat, but to provide for the generation of Spirits. And these alledge for themselves the Authority of the Antients, among whom *Hippocrates* seems manifestly to favor their Opinion; and both *Aristotle* and *Galen* do sometimes

(for methinks they ſpeak doubtfully enough) appear inclinable to it. But for ought ever I could ſee in Diſſections, it is very difficult to make out, how the Air is convey'd into the left Ventricle of the Heart, eſpecially the *Syſtole* and *Diaſtole* of the Heart and Lungs, being very far from being Synchronical: Beſides, that the Spirits ſeeming to be but the moſt ſubtle and unctuous Particles of the Blood, appear to be of a very differing Nature from that of the lean and incombuſtible Corpuſcles of Air. Other Objections againſt this Opinion have been propoſ'd, and preſt by that excellent Anatomiſt, and my Induſtrious Friend, Dr. *Highmore*, to whom I ſhall therefore refer you.

Another Opinion there is touching Reſpiration, which makes the genuine uſe of it to be Ventilation (not of the Heart, but) of the Blood, in its paſſage thorow the Lungs; in which paſſage, it is diſburthened of thoſe Excrementitious Steams, proceeding, for the moſt part, from the ſuperfluous Seroſities of the Blood, (we may adde) and of the *Chyle* too, which (by thoſe new Conduits of late very happily detected by the famous

Pecquet)

Pecquet) hath been newly mix'd with it in the Heart.) And this Opinion is that of the Industrious *Mœbius*, and is said to have been that of that excellent Philosopher *Gassendus*; and hath been in part an Opinion almost vulgar: But this *Hypothesis* may be explicated two ways: For first, The necessity of the Air in Respiration, may be suppos'd to proceed from hence; That as a Flame cannot long burn in a narrow and close place, because the Fuliginous Steams it uncessantly throws out, cannot be long receiv'd into the ambient Body; which after a while growing too full of them, to admit any more, stifles the flame, so that the vital Fire in the Heart requires an ambient Body, of a yielding nature, to receive into it the superfluous Serosities and other Recrements of the Blood, whose seasonable Expulsion is requisite to depurate the Mass of Blood, and make it fit both to Circulate, and to maintain the vital heat residing in the Heart. The other way of explicating the above-mention'd *Hypothesis*, is, by supposing, that the Air does not onely, as a Receptacle, admit into its Pores the Excrementitious vapors of the Blood, when they are expell'd through the Wind-Pipe,

but does alſo convey them out of the Lungs, in regard that the inſpired Air, reaching to all the ends of the *Aſpera Arteria*, does there aſſociate it ſelf with the Exhalations of the circulating Blood, and when 'tis exploded, carrys them away with it ſelf, as we ſee that winds ſpeedily dry up the ſurfaces of wet Bodies, not to ſay any thing of what we formerly obſervd touching our Liquor, whoſe fumes were ſtrangely elevated upon the Ingreſs of the Air.

Now of theſe two ways of explicating the uſe of Reſpiration, our Engine affords us this Objection againſt the firſt; That upon the Exſuction of the Air, the Animals die a great deal ſooner then if it were left in the Veſſel; though by that Exſuction the ambient ſpace is left much more free to receive the ſteams that are either breathed out of the Lungs of the Animal, or diſcharg'd by inſenſible Tranſpiration through the Pores of his Skin.

But if the *Hypotheſis* propoſ'd, be taken in the other ſenſe, it ſeems congruous enough to that grand obſervation, which partly the *Phænomena* of our Engine, and partly the relations of Travellers, have ſuggeſted to us, namely, That there is a

certain consistence of Air requisite to Respiration; so that if it be too thick, and already over-charged with vapors, it will be unfit to unite with, and carry off those of the Blood, as Water will dissolve, and associate to it self but a certain proportion of saline Corpuscles; and if it be too thin or rarefied, the number or size of the Aërial Particles is too small to be able to assume and carry off the halituous Excrements of the Blood, in such plenty as is requisite.

Now that Air too much thicken'd (and as it were clogg'd) with Steams, is unfit for Respiration, may appear by what is wont to happen in the Lead-Mines of *Devonshire*, (and, for ought I know, in those too of other Countrys, though I have seen Mines where no such thing was complain'd of) for I have been inform'd by more then one credible Person (and particularly by an Ingenious Man, that has often, for curiosity, digg'd in those Mines, and been imploy'd about them) that there often rises Damps, as retaining the *Germane* Word by which they call them) which does so thicken the Air, that unless the Work-men speedily make signs to them that are above, they would (which

alſo ſometimes happens) be preſently ſtifled for want of Breath; and though their Companions do make haſte to draw them up, yet frequently, by that time they come to the free Air, they are, as it were, in a ſwoon, and are a good while before they come to themſelves again. And that this ſwooning ſeems not to proceed from any Arſenical or Poyſonous Exhalation contain'd in the Damp, as from its over-much condenſing the Air, ſeems probable from hence; That the ſame Damps oftentimes leiſurely extinguiſh the flames of their Candles or Lamps; and from hence alſo that it appears (by many Relations of Authentical Authors) that in thoſe Cellars where great ſtore of new Wine is ſet to work, men have been ſuffocated by the too great plenty of the ſteams exhaling from the Muſt, and too much thickning the Air: as may be gathered from the cuſtom that is now uſed in ſome hot Countrys, where thoſe that have occaſion to go into ſuch Cellars, carry with them a quantity of well kindled Coals, which they hold near their Faces; whereby it comes to paſs, that the Fire diſcuſſing the Fumes, and rarefying the Air reduces the ambient Body to a conſiſtence fit for Reſpiration.

We will adde (by way of confirmation) the following Experiment: In ſuch a ſmall Receiver, as thoſe wherein we kill'd divers Birds, we carefully cloſ'd up one, who, though for a quarter of an hour he ſeem'd not much prejudiced by the cloſeneſs of his Priſon, afterwards began firſt to pant very vehemently, and keep his Bill very open, and then to appear very ſick; and laſt of all, after ſome long and violent ſtrainings, to caſt up ſome little matter out of his Stomack: which he did ſeveral times, till growing ſo ſick, that he ſtagger'd and gaſp'd, as being juſt ready to die; we perceiv'd, that within about three quarters of an hour from the time that he was put in, he had ſo thickned and tainted the Air with the Steams of his Body, that it was become altogether unfit for the uſe of Reſpiration: Which he will not much wonder at, who has taken notice in *Sanctorius* his *Statica Medicina*, how much that part of our Aliments, which goes off by inſenſible Tranſpiration, exceeds in weight all the viſible and groſſer Excrements both ſolid and liquid.

That (on the other ſide) an Air too much dilated is not ſerviceable for the ends of

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Reſpiration, the haſty death of the Animal we kill'd in our exhauſted Receiver, ſeems ſufficiently to manifeſt. And it may not irrationally be doubted, whether or no, if a Man were raiſ'd to the very top of the Atmoſphere, he would be able to live many minutes, and would not quickly dye for want of ſuch Air as we are wont to breath here below. And that this Conjecture may not appear extravagant, I ſhall on this occaſion ſubjoyn a memorable Relation that I have met with in the Learned *Joſephus Acoſta*, who tells us, That when he himſelf paſt the high Mountains of *Peru*, (which they call *Pariacaca*) to which, he ſays, That the *Alps* themſelves ſeem'd to them but as ordinary Houſes, in regard of high Towers, he and his Companions were ſurpriſed with ſuch extream Pangs of Straining and Vomiting, (not without caſting up Blood too) and with ſo violent a Diſtemper, that he concludes he ſhould undoubtedly have dyed, but that this laſted not above three or four hours, before they came into a more convenient and natural temperature of Air: To which our Learned Author addes an Inference, which being the principal thing I deſign'd in mentioning

tioning, the Narrative I ſhall ſet down in his own Words: *I therefore* (ſays he) *perſwade my ſelf, That the Element of the Air is there ſo ſubtle and delicate, as it is not proportionable with the breathing of Man, which requires a more groſs and temperate Air; and I believe it is the cauſe that doth ſo much alter the Stomack, and trouble all the Diſpoſition.* Thus far our Author, whoſe Words I mention, that we may gheſs by what happens ſomewhat near the Confines of the Atmoſphere (though probably far from the ſurface of it) what would happen beyond the Atmoſphere. That which ſome of thoſe that treat of the height of Mountains, relate out of *Ariſtotle*, namely, That thoſe that aſcend to the top of the Mountain *Olympus*, could not keep themſelves alive, without carrying with them wet Spunges, by whoſe aſſiſtance they could reſpire in that Air, otherwiſe too thin for Reſpiration: (That Relation (I ſay) concerning this Mountain) would much confirm what has been newly recited out of *Acoſta*, if we had ſufficient reaſon to belive it: But, I confeſs, I am very diffident of the truth of it; partly becauſe when I paſſ'd the *Alps*, I took notice of no notible change betwixt

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the consistence of the Air at the top and at the bottom of the Mountain; partly because of a very punctual relation made by an English Gentleman, of his ascension to the top of the Pike of *Tenariff* (which is by great odds higher then *Olympus*) I finde no mention of any such difficulty of breathing; and partly also because the same Author tells us out of *Aristotle*, That upon the top of *Olympus* there is no motion of the Air, insomuch, that Letters traced upon the dust, have been, after many years, found legible, and not discompos'd; whereas that Inquisitive *Busbequius* (who was Ambassador from the *German* to the *Turkish* Emperor) in one of his Eloquent Epistles, tells us, upon his own knowledge, *That* Olympus *may be seen from* Constantinople, *blanch'd with perpetual Snow*; which seems to argue, That the top of that, as well as of divers other tall Hills, is not above that Region of the Air wherein *Meteors* are formed. Though otherwise, in that memorable Narrative which *David Frælichius*, made of his ascent to the top of the prodigiously high *Hungarian* Mountain *Carpathus*: he tells us, *That when, having pass'd through very thick Clouds,*

Epist. 3.

Frælichius apud Varen: Geogra: Gener: lib. 1. *cap.* 19.

Clouds, he came to the very top of the Hill, he found the Air so calm and subtle, that not a hair of his head moved, whereas in the lower Stages of the Mountain he felt a vehement Wind. But this might well be casual, as was his, having a clear Air where he was, though there were Clouds, not onely beneath him, but above him.

But (though what has been hitherto discours'd, incline us to look upon the Ventilation and Depuration of the Blood, as one of the principal and constant uses of Respiration; yet) methinks it may be suspected that the Air does something more then barely help to carry off what is thrown out of the Blood in its passage through the Lungs, from the right Ventricle of the Heart to the left. For we see, in Phlegmatick Constitutions and Diseases, that the Blood will circulate tolerably well, notwithstanding its being excessively serous: And in Asthmatical Persons, we often see, that though the Lungs be very much stuff'd with tough Phlegm, yet the Patient may live some Moneths, if not some Years. So that

it ſeems ſcarce probable, that either the want of throwing out the ſuperfluous *Serum* of the Blood for a few Moments, or the detaining it, during ſo ſhort a while, in the Lungs, ſhould be able to kill a perfectly ſound and lively Animal: I ſay, for a few moments, becauſe, that having divers times try'd the Experiment of killing Birds in a ſmall Receiver, we commonly found, that within half a minute of an hour, or thereabouts, the Bird would be ſurpriſ'd by mortal Convulſions, and within about a minute more would be ſtark dead, beyond the Recovery of the Air, though never ſo haſtily let in. Which ſort of Experiments ſeem ſo ſtrange, that we were oblig'd to make it ſeveral times, which gain'd it the Advantage of having Perſons of differing Qualities, Profeſſions and Sexes, (as not onely Ladies and Lords, but Doctors and Mathematicians) to witneſs it. And to ſatisfie Your Lordſhip, that it was not the narrowneſs of the Veſſel, but the ſudden Exſuction of the Air that diſpatch'd theſe Creatures ſo ſoon; we will adde, That we once incloſ'd one of theſe Birds in one of theſe ſmall Receivers, where, for a while, he was ſo little ſenſible of his Impriſonment, that he

he eat very chearfully certain Seeds that we convey'd in with him, and not onely liv'd ten minutes, but had probably liv'd much longer, had not a great Person, that was Spectator of some of these Experiments, rescu'd him from the prosecution of the Tryal. Another Bird being within about half a minute, cast into violent Convulsions, and reduc'd into a sprawling condition, upon the Exsuction of the Air, by the pitty of some Fair Lady's (related to Your Lordship) who made me hastily let in some Air at the Stop-cock, the gasping Animal was presently recover'd, and in a condition to enjoy the benefit of the Lady's Compassion. And another time also, being resolv'd not to be interrupted in our Experiment, we did, at night, shut up a Bird in one of our small Receivers, and observ'd, that for a good while he so little felt the alteration of the Air, that he fell asleep with his head under his wing; and though he afterwards awak'd sick, yet he continu'd upon his legs between forty minutes and three quarters of an hour; after which, seeming ready to expire, we took him out, and soon found him able to make use of the liberty we gave him for a compensation of his sufferings.

If to the foregoing Instances of the sudden destruction of Animals, by the removal of the ambient Air, we should now annex some, that we think fitter to reserve till anon; perhaps Your Lordship would suspect, with me, that there is some use of the Air, which we do not yet so well understand, that makes it so continually needful to the Life of Animals. *Paracelsus* indeed tells us, *That as the Stomack concocts Meat, and makes part of it useful to the Body, rejecting the other part, so the Lungs consume part of the Air, and proscribes the rest.* So that according to our Hermetick Philosopher (as his followers would have him stil'd) it seems we may suppose, that there is in the Air a little vital Quintessence (if I may so call it) which serves to the refreshment and restauration of our vital Spirits, for which use the grosser and incomparably greater part of the Air being unserviceable, it need not seem strange that an Animal stands in need of almost incessantly drawing in fresh Air. But though this Opinion is not (as of some of the same Author) absur'd, yet besides that, it should not be barely asserted, but explicated and prov'd; and besides that, some Objections may be

fram'd

fram'd againſt it, out of what has been already argu'd againſt the Tranſmutation of Air into vital Spirits: Beſides theſe things, it ſeems not probable, that the bare want of the Generation of the wonted quantity of vital Spirits, for leſs then one minute, ſhould within that time be able to kill a lively Animal, without the help of any external violence at all.

But yet, on occaſion of this Opinion of *Paracelſus*, perhaps it will not be impertinent, if before I proceed, I acquaint Your Lordſhip with a Conceit of that deſervedly Famous Mechanician and Chymiſt, *Cornelius Drebell*, who among other ſtrange things that he perform'd, is affirm'd (by more then a few credible Perſons) to have contriv'd for the late Learned King *James*, a Veſſel to go under Water; of which, tryal was made in the *Thames*, with admired ſucceſs, the Veſſel carrying twelve Rowers, beſides Paſſengers; one of which is yet alive, and related it to an excellent Mathematician that inform'd me of it. Now that for which I mention this Story, is, That having had the curioſity and opportunity to make particular Enquiries among the Relations of *Drebell*, and eſpecially of an Ingenious

ſitian that marry'd his daughter, concerning the grounds upon which he conceived it feaſible to make men unaccuſtom'd to continue ſo long under water without ſuffocation, or (as the laſtly mention'd Perſon that went in the veſſell affirmes) without inconvenience. I was anſwer'd, that *Drebell* conceiv'd, that 'tis not the whole body of the Air, but a certain Quinteſſence (as Chymiſts ſpeake) or ſpiritu-ous part of it, that makes it fit for reſpiration, which being ſpent, the remaining groſſer body, or carcaſe (if I may ſo call it) of the Air, is unable to cheriſh the vitall flame reſiding in the heart: So that (for ought I could gather) beſides the Mechanicall contrivance of his veſſell he had a Chymicall liquor, which he accounted the chiefe Secret of his ſubmarine Navigation. For when from time to time he perceiv'd, that the finer and purer part of the Air was conſum'd, or over clogg'd by the reſpiration, and ſteames of thoſe that went in his ſhip, he would, by unſtopping a veſſell full of this liquor, ſpeedily reſtore to the troubled Air ſuch a proportion of Vitall parts, as would make it againe, for a good while, fit for Reſpiration, whether by diſſipating, or precipitating the

groſſe

grosser Exhalations, or by some other intelligible way, I must not now stay to examine: Contenting my selfe to add, that having had the opportunity to do some service to those of his Relations, that were most Intimate with him, and having made it my business to learne what this strange Liquor might be, they constantly affirm'd that *Drebell* would never disclose the Liquor unto any, nor so much as tell the matter whereof he made it, to above one Person, who himselfe assur'd me that it was.

This account of *Drebell's* performance, I mention, not that I any further assent to his opinion then I have already intimated, but because the man, and the Invention being extraordinary, I suppose Your Lordship will not be displeas'd to know the utmost I could learne about it; especially not having found it mention'd by any Writer. Wherefore I have been sometimes inclin'd to favourable thoughts of their opinion, who would have the Aire necessary to ventilate, and cherish the vitall flame, which they do suppose to be continually burning in the heart. For we see, that in our Engine the flame of a Lamp will last almost as little after the Exsuction

on of the Air, as the life of an Animall: Nay I remember, that though I devis'd a more promising way, to make a fire last in our exhausted Receiver, yet it would not succeed: We tooke a hard body made in the forme of a Clove, but twice as long, and proportionably thick, this body being made of such a Composition, that if it be kindl'd at the upper end, it will most certainly burn away to the very bottome, much better then a Match; we convey'd it diverse times kindl'd at the upper end, into one of our small Receivers, but still found, that though presently upon the Exsuction of the Air, it would leave smoaking, and seeme quite gone out, and againe begin to smoke as soon as the Air was let in upon it; yet if the Air were kept out but foure or five minutes, the fire would be totally, and irrevocably extinguish'd. To which wee will adde, that though we convey'd into a great Receiver, a small lamp with rectifi'd spirit of Wine, that being so pure as not to smut the Cotton weeke, or so much as a piece of white Paper held over it; yet we could not by divers tryalls make the flame last a couple of minutes after the Air was begun to be drawne out. But though our Engine thus

thus shews us a new kind of resemblance betwixt fire and life: yet the opinion we have last mention'd is not free from Difficulties. For though in the hearts of many Animall's Blood be a warm liquor, and in some ev'n a hot one; yet it is not easie to conceive either how the Air (in substance) can get thither, or how, in case it could, it were able to encrease the heat. Since, however, the Air may encrease the heat of a coale by blowing off the ashes, and making the active Corpuscles pierce further into the kindl'd body, and shatter it the more, yet we see hot liquors have their heat allay'd, and not augmented, by having Air blown on them. And whereas some Eminent Naturalists think it not inconvenient, to make the heat residing in the heart to be a true flame, provided they adde, that 'tis such a temperate, and almost insensible fire, as the flame of spirit of Wine, which will long burne upon fine white Linnen or Paper without consuming either: give mee leave to wish that they had been more curious to make differing trials with that liquor. For (as we observe in another Treatise) the reason why a Linnen cloth, dipp'd in common Spirit of Wine, is not burnt by the flame of it, is

because the Phlegm of the Liquor defends the Cloth. And the Flame of Spirit of Wine is ſo far from being too weak to burn a piece of Paper, or of Linnen, that I have uſ'd it in Lamps to diſtill Liquors out of tall Cucurbits, and found that the Spirit burn'd away indeed much faſter then Sallet Oyl, but gave at leaſt as great a heat: Nay, I have, for curioſity ſake, melted crude Gold, and that readily enough, with the bare Flame of pure Spirit of Wine.

But not to preſs this any further, we will, on this occaſion, venture to ſubjoyn an odde Obſervation, which may perhaps invite to a further Enquiry into the Opinion we have for Diſcourſe ſake oppoſ'd. Our Engliſh *Democritus*, Dr. *Harvey*, propoſes this difficult and noble Problem to Anatomiſts, *Why a* fætus, *even out of the Womb, if involv'd in the ſecundines, may live a good while without Reſpiration; but in caſe after having once begun to breath, its reſpiration be ſtopp'd, it will preſently die.* We are far from pretending to ſolve ſo hard a Problem, but this we try'd in relation to it; We took a Bitch that was ſaid to be almoſt ready to whelp, and having cauſ'd her to be hang'd, we preſently open'd

open'd her *Abdomen*, and found four Puppeys in her Womb; one of these we took out, and having freed him from the Teguments that involv'd him, and from the Liquor he swam in, we observ'd that he quickly open'd his Mouth very wide, mov'd his Tongue, and exercis'd Respiration; then we open'd both his *Abdomen* and his Chest, and cut assunder the *Diaphragme*, notwithstanding which, he seem'd often to endeavor Respiring, and mov'd in a notable manner, both the Intercostal Muscles, part of the *Diaphragme*, the Mouth and the Tongue: But that which we mention this Puppy for, was this, That being desirous to try whether the other yong ones that had not yet breath'd at all, would long survive this or no; we took them also out of the Womb, and having open'd them, found none of them so much alive, as to have any perceptible motion in his heart, whereas the heart of that Puppy which had once enjoy'd the benefit of Respiration, continu'd beating so long, that we our selves observ'd the Auricle to beat, after five or six hours; and a Servant that staid up and watch'd it after we were gone to Bed, affirm'd, That he saw the Pulsation conti-

nue about two hours longer. I ſhall leave it to others to make Reflections upon this Obſervation, compar'd with Dr. *Harvey*'s Problem.

It is much doubted, whether Fiſhes breath under Water, and we ſhall not take upon us, as yet, to determine the Queſtion either way, becauſe we have not yet been able to procure little Fiſhes alive to make Experiments upon: That ſuch as are not Setaceous (for ſuch manifeſtly breath) have not Reſpiration, properly ſo call'd, ſuch as is exerciſ'd by four footed Beaſts, and Birds, may be argu'd from their having but one cavity in their Hearts, & from their want of Lungs, whence they are obſerv'd to be Mute; unleſs we ſay, what is not altogether abſurd, That their Gills ſeem ſomewhat Analogous (as to their uſe) to Lungs. But that on the other ſide, Air is neceſſary to the Lives even of Fiſhes, and that thereſore 'tis probable they have ſome obſcure kinde of Reſpiration, ſeems manifeſt by two or three Obſervations and Experiments, mention'd by divers Authors, who tell us, *That Fiſhes ſoon die in Ponds and Glaſſes quite fill'd with Water; if the one be ſo frozen over, and the other ſo cloſely ſtopp'd, that the Fiſhes cannot enjoy the benefit of the Air*

Air, if we allow them to be true. But because these Relations are not wont to be deliver'd by Writers upon their own Knowledge; as I shall not reject them, so I dare not build upon them, till I have opportunity to examine them by experience. In the mean time, we will adde, That our Engine has taught us two things that may illustrate the matter in hand: The one, That there is wont to lurk in Water, many little parcels of intersperſ'd Air, whereof it seems not impossible that Fishes may make some use, either by separating it when they strain the Water thorow their Gills, or by some other way: The other, what may be collected from the following Experiment.

We took a large Eele (being able to procure no other Fish alive) and removing it out of the Vessel of Water, wherein it was brought us, into our great Receiver, we caus'd the Air to be pump'd out; and observ'd, That the Eele, after some motion to and fro in the Glass, seem'd somewhat dis-compos'd; and that when we had prosecuted the Exsuction of the Air somewhat obstinately, she turn'd up her Belly, as dying Fishes are wont to do, and from thence-forward lay altogether

moveless,

moveleſs, juſt as if ſhe were ſtark dead; and though I did not think her ſo, yet the continuing in that Poſture, even after the Cover of the Receiver was taken off (whereby the Air was let in) I ſhould have been of the Opinion of the By-ſtanders, if the Diffidence I am wont to exerciſe in trying Experiments (eſpecially ſuch as are not uſual) had not invited me to take the Fiſh out of the Receiver, upon which ſhe ſhew'd her ſelf, by her vivid motions, as much alive as before.

But that is moſt ſtrange which we obſerv'd of a great, gray, Houſe-Snail (as they call it) which being cloſ'd up in one of our ſmall Receivers, did not onely, not fall down from the ſide of the Glaſs, upon the drawing-out of the Air (For that may be aſcrib'd to the tenacity of the Liquor wherewith Snails uſe to ſtick themſelves, even to the ſmootheſt Bodies) but was not ſo much as depriv'd of progreſſive motion by the receſs of the Air: Though except this Snail, we never put any living Creature into our Receiver, whom it did not either kill, or at leaſt reduce to ſeem ready to dye. But as we ſhall not here examine what intereſt the glutinous, and uneaſily diſſipable Na-

ture

ture of the Juices of Snails, may have on this event; so whether this escape of our Eele be to be ascrib'd to the particular and vivacious Nature of this sort of Fishes; or to this, That the Air is not indeed necessary to the life of Fishes; or finally to this, That though these Animals need some Air, yet they need so little, that that which could not be drawn out of the Receiver, might (at least for a while) suffice them, we will not now determine.

Nor are we at leisure to examine that Paradox of *Hippocrates*, which some Learned Physitians have of late reviv'd, namely, That the *Fœtus* respires in the Womb: For on the one side it seems very difficult to conceive, how Air should traverse the Body of the Mother, and the Teguments of the Childe: And since Nature has, in new-born Babes, contriv'd peculiar and temporary Vessels, that the Blood may circulate thorow other Passages, then it is wont to do in the same Individuals when they come to have the free use of their Lungs, it seems unlikely, that Infants in the Womb do properly respire. But then since our Experiments have manifested, That almost all kinde of Liquors do, as well as Water, abound with in-

interſperſ'd Corpuſcles of Air, it ſeems not altogether abſurd to ſay, That when the *Fœtus* is grown big, he may (eſpecially the upper part of the involving *Amnios*, being deſtitute of Liquor, and fill'd onely with an halituous Subſtance) exerciſe ſome obſcure Reſpiration, eſpecially, ſince 'tis not (as many wiſe Men think it) a Fable, That Children have been heard to cry in the Mothers Womb: For though it happens exceeding rarely, yet ſometimes it has been obſerv'd. And I know a young Lady, whoſe Friends, when ſhe was ſome Years ſince with Childe, complain'd to me, That ſhe was ſeveral times much frighted with the Cryes of her Infant, which, till I diſabuſ'd Her, She and Her Friends look'd upon as Portentous. And ſuch Obſervations are the more credible, becauſe not onely Houſwives, but more judicious Perſons, mention it, as no very unfrequent thing to hear the Chick Pip or Cry in the Egg, before the Shell be broken. But this I mention but as a probable, not a cogent Argument, till I can diſcover whether an Eliſion of an halituous Subſtance, though noe true Air, may not at the top of the *Larynx* pro-

produce a Sound, ſince I find that the Blade of a Knife, held in ſeverall poſtures in the ſtreame of Vapors (or rarified Water) that iſſu's out of an *Æolipile*, will afford various and very audible Sounds.

I had thoughts of conveying into our Receiver young ones, ripped out of the wombe of their Dammes, with their involving Coates intire, but could not procure them. And I have alſo had thoughts of trying whether it be not practicable, to make a Receiver, though not all of glaſſe, yet with little glaſſe windows, ſo placed, that one may freely look into it, capacious enough to hold a Man, who may obſerve ſeverall things, both touching Reſpiration, and divers other matters; and, who in caſe of fainting, may, by giving a ſigne of his weakneſſe, be immediately reliev'd by having air let in upon him. And it ſeems not impoſſible, but that by accuſtomance, ſome Men may bring themſelves to ſupport the want of Air a pretty while, ſince we ſee that divers will live ſo much longer then other Men under Water: that thoſe that dive for Pearles in the Weſt Indies are ſaid to be able to ſtay a whole houre under water. And *Cardan* tels us of one *Colanus* a Diver in *Sicily*, who was

able

Cardan. de Subtilitat. lib. 11. able to continue (if *Cardan* neither mistake, nor impose upon us) three or foure times as long. Not to mind Your Lordship, that You have Your selfe often seen in *England*, a corpulent Man, who is wont to descend to the bottome of the *Thames*, and bring out of the deep holes at the bottome of the bankes, large fishes alive in his hands. And *Acosta* tels us, he saw in *Peru* the like manner of fishing, but more difficult, practised by the *Indians*. lib. 3. c. 15.

I made mention of some Men, and of *Accustomance*: because there are but very few, who, though they use themselves to it by degrees, are fit to support, for many Minutes, the want of Air. Insomuch that an ingenious Man of my acquaintance, who is very famous for the usefull skill of drawing Goods, and ev'n Ordnance out of sunke Ships, being asked by mee, how long he was able to continue at the depth of 50. or 60. feet under water, without the use of Respiration, confessed to mee, that hee cannot continue above two minutes of an houre, without resorting to the Air, which he carries downe with him in a certaine Engine (whereof I can show your Lordship a Description.) Another thing I also learn'd of him by enquiry, that was

not

not despicable: For asking him, whether he found any use of chawing little sponges dipt in oyle in his Mouth, when he was perfectly under water, and at a distance from his Engine, he told me, that by the help of these sponges he could much longer support the want of his wonted Respiration, then he was able to do without them. The true cause of which would perhaps, if discovered, teach us some thing pertinent to the Probleme touching the Respiration of Fishes

But the necessity of Air to the most part of Animals unaccustom'd to the want of it, may best be judg'd of by the following Experiments, which we try'd in our Engine, to discover whether Insects themselves have not, either Respiration, or some other use of the Air equivalent thereunto.

We tooke then an humble-bee, one of those common flyes that are call'd flesh flyes, and one of those hairy wormes that resemble caterpillars, and are wont to be call'd Palmer-wormes: These three wee convey'd into one of our small Receivers, and observ'd to the great wonder of the Beholders, that not onely the Bee, and the Fly fell downe, and lay with their bellies

upwards;

upwards; but the worme it selfe seem'd to be suddenly struck dead: all of them being reduc'd to lye without motion, or any other discernable signe of life, within somewhat lesse (if we mistake not) then one minute of an houre: And this, notwithstanding the smalnesse of the Animals in proportion to the capacity of the vessels: which circumstance we the rather mention, because we found that the vessell was not free from leaks. And to satisfie the Spectators, that 'twas the absence of the Air that caus'd this great and sudden change: we had no sooner re-admitted the Air at the stopcock, than all the three Insects began to shew signes of life, and little by little to recover. But when we had again drawn out the Air, their motions presently ceased, & they fell down seemingly dead as before, cōtinuing moveless, as long as, by continuing to pump, the vessell was kept exhausted. This invited us thankfully to reflect upon the wise goodnesse of the Creator, who by giving the Air a spring, has made it so very difficult, as men find it, to exclude a thing so necessary to Animals: and it gave us also occasion to suspect that if Insects have no lungs, nor any part analogous thereunto, the ambient

Air

Air affects them, and relieves them at the Pores of their Skin, it not being irrational to extend to these Creatures that of *Hippocrates*; who says, That a Living Body is throughout perspirable; or to use his expression, εἴσπνουν καὶ ἔκπνουν, dispos'd to admit and part with what is Spirituous: Which may be somewhat Illustrated by what we have elsewhere noted, That the moister parts of the Air readily insinuate themselves into, and recede from the pores of the Beards of wilde Oates, and those of divers other wilde Plants; which almost continually wreath and unwreath themselves according to, even, the light variations of the temperature of the ambient Air.

This Circumstance of our Experiment we particularly took notice of, that when at any time, upon the Ingress of the Air, the Bee began to recover, the first sign of Life she gave, was a vehement panting, which appear'd near the Tail: Which we therefore mention, because we have observ'd the like in Bees drown'd in Water, when they first come to be reviv'd by a convenient heat: As if the Air were in the one case as proper to set the Spirits

and Alimental Juice a moving, as heat is in the other; and this may perchance deserve a further consideration.

We may adde, That we scarce ever saw any thing that seem'd so much as this Experiment, to manifest, That even living Creatures (Man always excepted) are a kinde of curious Engines, fram'd and contriv'd by nature (or rather the Author of it) much more skilfully then our gross Tools and unperfect Wits can reach to. For in our present Instance we see Animals, vivid and perfectly sound, depriv'd immediately of motion, and any discernable signs of life, and reduc'd to a condition that differs from death, but in that it is not absolutely irrecoverable. This (I say) we see perform'd without any, so much as the least external violence offered to the Engine; unless it be such as is offer'd to a Wind-Mill, when the Wind ceasing to blow on the Sayls, all the several parts remain moveless and useless, till a new Breath put them into motion again.

And this was further very notable in this Experiment; That whereas tis known, that Bees and Flies will not onely walk, but flie for a great while, after their heads

are

are off; and sometimes one half of the Body will, for divers hours, walk up and down, when it is sever'd from the other: Yet, upon the Exsuction of the Air, not onely the progressive motion of the whole Body, but the very motions of the Limbs do forthwith cease; as if the presence of the Air were more necessary to these Animals, then the presence of their own Heads.

But it seems, that in these Insects, that fluid Body (whether it be a Juice or Flame) wherein Life chiefly resides, is nothing near so easily dissipable, as in perfect Animals. For where, as we have above recited, that the Birds we conveyed into our small Receiver were within two minutes brought to be past Recovery, we were unable (though by tyring him that pump'd) to kill our Insects by the exsuction of the Air: For though, as long as the Pump was kept moving, they continued immovable, yet when we desisted from pumping, the Air that press'd in at the unperceiv'd Leaks, did (though slowly) restore them to the free exercise of the functions of Life.

But, My Lord, I grow troublesome, and therefore shall pass on to other Expe-

periments: Yet without dispairing of your pardon for having entertain'd you so long about the use of Respiration, because it is a subject of that difficulty to be explain'd, and yet of that importance to humane Life, that I shall not regret the trouble my Experiments have cost me, if they be found in any degree serviceable to the purposes to which they were design'd. And though I despair not but that hereafter our Engine may furnish us with divers *Phænomena* useful to Illustrate the Doctrine of Respiration; yet having not, as yet, had the opportunity to make the other tryals, of various kinds, that I judge requisite for my Information: I must confess to Your Lordship, that in what I have hitherto said, I pretend not so much to establish, or over-throw this or that *Hypothesis*, as to lay together divers of the Particulars that occur'd to me, in order to a future inquiry. I say, divers of the Particulars, because I could adde many others, but that I want time, and fear that I shall need Your Lordships pardon, for having been so prolix in Writing; and that of Physitians (which perhaps I shall more easily obtain) for having invaded Anatomy, a Discipline which

they

they challenge to themselves, and indeed have been the almost sole Improvers of. Without denying then that the inspir'd and exspir'd Air may be sometimes very useful, by condensing and cooling the Blood that passes through the Lungs; I hold that the depuration of the Blood in that passage, is not onely one of the ordinary, but one of the principal uses of Respiration. But I am apt also to suspect, that the Air does something else in Respiration, which has not yet been sufficiently explain'd; and therefore, till I have examin'd the matter more deliberately, I shall not scruple to answer the Questions that may be asked me touching the genuine use of Respiration, in the excellent Words employ'd by the acute St. *Austin*, to one that ask'd him hard Questions: *Mallem quidem* (says he) *eorum que à me quæsivisti, habere scientiam quam ignorantiam: sed quia id nondum potui, magis eligo cautam ignorantiam confiteri, quam falsam scientiam profiteri.*

Experiment 42.

HAving (partly upon the conſideration of ſome of the foregoing Experiments, and partly upon grounds not now to be inſiſted on) entertain'd a ſuſpition, that the action of Corroſive Liquors in the diſſolving of Bodies, may be conſiderably varied by the gravitation or preſſure of the incumbent Air, and the removal of it; I thought fit to examine my Conjecture by the following Experiment.

I took whole pieces of red Coral, and caſt them into as much Spirit of Vinager, as ſufficed to ſwim above an Inch over them: Theſe ſubſtances I made choice of, that the Ebullition upon the Solution might not be too great, and that the operation might laſt the longer.

Having then put about half-a-ſcore Sprigs of Coral, together with the *Menſtruum*, into a ſomewhat long neck'd Viol, whereof they ſeem'd ſcarce to fill a third part, we convey'd that Viol into one of our ſmall *Pneumatical* Glaſſes, containing by gheſs about a Quart of Water; and having faſtned on the Cover, after the accuſtom'd manner, we ſuffered the Liquor

quor to remain unmov'd awhile, to observe whether the *Menstruum* would work upon the Coral otherwise then before. But finding there did onely arise, as formerly, a pretty number of small Bubbles, that made no sensible froth upon the surface of the distill'd Vinager, there were made two or three Exsuctions of the Air; upon which, there emerg'd from the Corall such a multitude of Bubbles, as made the whole Body of the *Menstruum* appear white; and soon after, a Froth, as big as all the rest of the Liquor, was seen to swim upon it: And the *Menstruum* plainly appear'd to boil in the Glass, like a seething Pot. And though, if we desisted but one minute from pumping, the decrement of the Froth and Ebullition, upon the getting in of a little Air at some leak or other, seem'd to argue, that the removal of the the pressure of the external Air was the cause, or, at least, the occasion of this effervescence: Yet to evince this the more clearly, we turn'd the Key, and let in the external Air at the Stop-cock; immediately upon whose entrance the Froth vanish'd, and so many of the Bubbles with-

 in

in the body of the Liquor disappear'd, that it lost its whiteness, & grew transparent again: The *Menstruum* also working as languidly upon the coral, as it did before they were put into the Receiver: But when we had again drawn out the Air, first the whiteness re-appear'd, then the ebulition was renewd, which, the pumping being awhile longer & nimbly pursued, grew so great, that for 3 or 4 times one after another, when ever the Air was let out of the Receiver into the emptyed Cylinder; the frothy liquor over-flow'd the glass, & ran down by the sides of it: And yet, upon the readmitting of the excluded air, the boiling Liquor grew immediatly as calm and as transparent as at first: as if indeed the operation of it, upon the Coral, had been facilitated by the exsuction of the incumbent air, wch on its recess, left it more easie for the more active parts of the liquor to shew themselves such, then it was whilst the wonted pressure of the Air continued unremoved. It may indeed be suspected, that those vast & numerous Bubbles proceeded, not from the action of the *Menstruum* upon the Corall, but from the suddain emersion of those many little parcels of air that (as we formerly observd) are wont to be dispers'd in liquors, without excluding Spirit of Vinegar; but having had this suspition before we tryd the Experiment, we convey'd

vey'd our diſtill'd Vinager alone into the Receiver, and kept it awhile there, to free it from its Bubbles (which were but very ſmall) before ever we put the Corall into it. It may be ſuſpected likewiſe, that the agitation of the Liquor, neceſſary following upon the ſhaking of the Glaſs, by pumping, might occaſion the recited Ebullition; but upon tryal made, there appear'd not any notable change in the liquor, or its operation, though the containing Veſſel were ſhaken, provided no Air were ſuck'd out of it. The former Experiment was another time tryd in another ſmall Receiver, with Coral groſly poudred, and the ſucceſs was very much alike, ſcarce differing in any thing, but that the Coral being reduc'd to ſmaller parts, upon the ebullition of the liquor, ſo many little lumpsof Coral would be carryed & Boy'd up by the emerging Bubbles, as ſometimes to darken the Viol; though the ſame Coralline Corpuſcles would be let fall again upon the letting in of the Air.

Something alſo we try'd in our great Receiver, concerning the ſolution of Metals in *Aqua fortis*, and other Corroſive Liquors; but partly the ſtink, and partly ſome accidents, kept us from obſerving any thing peculiar & remarkable about thoſe Solutions.

One thing we muſt not omit, that when the Spirit of Vinager was boiling upon the Coral, we took off the Cover of the Receiver, and took out the Viol, but could not finde, that notwithſtanding ſo very late an Ebullition, the Liquor had any heat great enough to be at all ſenſible to our hands.

We

Experiment 43. WE will now ſubjoyn an Experiment, which, if the former did not leſſen, the wonder of it would probably appear very ſtrange to Your Lordſhip, as it did to the firſt Spectators of it.

The Experiment was this: We cauſ'd Water to be boyl'd a pretty while, that by the heat it might be freed from the latitant Air, ſo often already taken notice of in common Water: Then almoſt filling with it a Glaſs Viol, capable of containing near four Ounces of that Liquor; we convey'd it, whil'ſt the Water was yet hot, into one of our ſmall Receivers (big enough to hold about a pound of Water) and having luted on the Cover, we cauſ'd the Air to be drawn out: Upon the two firſt Exſuctions, there ſcarce appear'd any change in the Liquor, nor was there any notable alteration made by the third; but at the fourth, and afterwards, the Water appear'd to boyl in the Viol, as if it had ſtood over a very quick Fire; for the Bubbles were much greater then are uſually found upon the Ebullition of very much

much more Water then was contain'd in our Viol. And this Effervescence was so great in the upper part of the Water, that the Liquor boyling over the top of the Neck a pretty deal of it ran down into the Receiver, and sometimes continued (though more languidly) boyling there. Prosecuting this Experiment, we observ'd, that sometimes, after the first Ebullition, we were reduc'd to make divers Exsuctions of the Air, before the Liquor would be brought to boyl again. But at other times, as often as the Key was turn'd to let the Air pass from the Receiver into the Pump, the Effervescence would begin afresh, though the Pump were ply'd for a pretty while together; which seem'd to argue, that the boyling of the Water proceeded from hence, That upon the withdrawing the pressure of the incumbent Air, either the Fiery Corpuscles, or rather the Vapors agitated by the heat in the Water (which last, what we have formerly noted touching the rarefied Water of an *Æolipile*, manifest to be capable of an Elastical Power) were permitted to expand themselves mightily in the evacuated Receiver; and did, in their

tumul-

tumultuous Dilation, lift up (as the Air is wont to do) the uppermost part of the Water, and turning it into Bubbles, made the Water appear boiling. This conjecture was further confirm'd by these additional Circumstances: First, The Effervescence was confin'd to the upper part of the Water, the lower remaining quiet, unless the Liquor were but shallow. Next, although sometimes (as is already noted) the Ebullition began again, after it had ceas'd a pretty while, which seem'd to infer, That some concurrent cause (whatever that were) did a little Modifie the operation of heat; yet, when the water in the Viol could by no pumping be brought to boil any more, the self-same Water, being in the very same Viol warm'd again, and reconvey'd into the Pneumatical Glass, was quickly brought to boyl afresh, and that vehemently and long enough; not to mention, that a new parcel, taken out of the same parcel of the boyled Water with the former, and put in cold, could by no pumping be brought to the least shew of Effervescence. Besides, having try'd this Experiment in hot Sallet Oyl, being

ing a much more tenacious Liquor, and requiring a ſtronger heat to make it boil, could not be brought to an Efferveſcence in our Reciver; whereas the Chymical Oyl of Turpentine, being thinner and more volatile, was preſently made to boyl up, till it reach'd four or five times its former height in the Viol, in whoſe bottom it lay, and continu'd boyling till it was almoſt reduc'd to be but luke-warm. Wine alſo being a more thin and ſpirituous Liquor then Water, being convey'd in hot inſtead of the Oyl, did, as I remember, at the very firſt Exſuction begin to boyl ſo vehemently, that, in a ſhort time that the Pump was kept moving, four parts of five, by our gheſs, boyl'd over out of the Viol, though it had a pretty long Neck. On which occaſion we will adde, that even the Water it ſelf, near one half, would ſometimes boyl over into the Receiver before it became luke-warm. And it was alſo remarkable, that once, when the Air had been drawn out, the Liquor did, upon a ſingle Exſuction, boyl ſo long with prodigiouſly vaſt Bubbles, that the Efferveſcence laſted almoſt

most as long as was requisite for the rehearsing of a *Pater Noster*. Now the Experiment having been try'd more then once, and found to succeed as to the main, seems much to countenance the conjecture we made at the beginning of this Letter, where we told your Lordship, That perhaps the pressure of the Air might have an interest in more *Phænomena* then men have hitherto thought. For as we had not then made this Experiment, so now we have made it, it seems to teach, That the Air, by its stronger or weaker pressure, may very much Modifie (as the School-men speak) divers of the Operations of that vehement and tumultuous Agitation of the small parts of Bodies, wherein the nature of heat seems chiefly, if not solely, to consist. Insomuch that if a heated Body were convey'd above the Atmosphere, 'tis probable that the heat may have a differing operation, as to the power of dissipating the parts of it, from what it has here below.

To conclude, This Experiment might have been further prosecuted, but our want of leasure makes us content our selves to adde at present; That perhaps

it would not be lost labor if this were try'd, not onely with other Liquors, but with variety of heated, and especially soft or melted Bodies: But in such cases the Receiver ought to be so shap'd, as is most proper to preserve the Cement wherewith the Cover must be fastned on, from being melted by the heat of the included Matter; the inconvenience to be hereby avoided, having befallen us in the use of a Receiver too shallow, though otherwise capacious enough.

The

The Conclusion.

BEing come thus far, My Dear Lord, not without thoughts of proceeding further: The unwelcome Importunity of my Occasions becomes so prevalent, that it quite hinders, for the present, my design'd Progress; and reduces me, not onely to reserve for another opportunity that kinde of Experiments, which, at some distance from the beginning of this Letter, I call'd (as Your Lordship may remember) Experiments of the second sort; but to leave unessay'd some of the first sort, which I might try in the Engine, as it now is, were it not that my Avocations are grown so urgent, for my remove from the place where the Engine was set up, that I am put to write Your Lordship this Excuse, Weary, and in an Inne which I take in my way to my Dear-

rest Brother *Corke*: Who being at length arriv'd in *England*, after I have for diverse Yeares been deprived of His Company, and wish'd for it as long; what ever my other occasions may be, my first Businesse must be to wait on Him and Your Excellent Mother; in whose gratefull Company I may hope to forget a while those publick calamities that distresse this too deservedly unhappy Nation. Since that is indear'd to me, both by their personall Merit; by the near Relation which Nature gives me to Him, Affinity to Her, and Friendship to both; and also by their many Favours, especially that of my owing them My *Lord of Dungarvan*. But I suffer my selfe to be transported too farre with these delightfull thoughts; To returne therefore to our Engine. Though I find this Letter is beyond my expectation swell'd, not only into a Book, but almost into a Volume; yet the Experiments already mentioned in it, are so farre from comprising all those that may be try'd by the help of our Engine, that I have not yet been able to try all those, which, presently occurring

to my thoughts, upon my first seeing the working of it, I Caus'd to be set down in a Catalogue within lesse then halfe an houre. But I doubt I have but too much cause. to apprehend that the Affaires, and other things I complaine of, have made it needfull for me to Apologize, as well for the things I have set down, as for those I am necessitated to omit. For as partiall as men use to be to the children of their own Braines, as well as to those of their Loines, I must not deny that the foregoing Tryals are not altogether free from such unaccuratenesses, nor the recitall of them from such imperfections, as I my selfe can now discerne, and could perhaps partly mend, if I had the leasure to repeate the Experiments, with the Circumstances that have since offer'd themselves to my thoughts, as things that might have been worth Observation or Enquiry. But the truth is, that I was reduc'd to make these Experiments, when my Thoughts had things that more concernd me to imploy them, and the same avocations made me set them down, for the most part, assoone as I had

had made them, and in the ſame order, and that ſo faſt that I had not over-frequently the opportunity to mind any more then the bare Truth of what I ſet down; without allowing it any of thoſe Advantages that Method, Style, and decent Embelliſhments, are wont to conferre on the Compoſures they are imploy'd do adorne.

But, my Lord, though to invite and encourage You and your learned Friends at *Paris*, to make a further uſe of this Engine than I have yet been able to do, I am thus free to acknowledge the imperfections of the foregoing Letter: yet if ſome Intelligent Perſons miſtake not, by what has been done, ſuch as it is, there is a way open'd, whereby Sagacious Wits will be aſſiſted to make ſuch further Diſcoveries in ſome points of Naturall Philoſophy, as are yet ſcarce dream'd of. And I am the more deſirous to engage You to that Imployment, becauſe I am apt to think, that if the Making and Writing of ſuch Experiments ſhall coſt You as much trouble as they have me, You will be inclin'd to Excuſe me; and if the Diſcoveries give You

as much pleasure as they gave me, You will (perhaps) be invited to thank me. However, I think (my Lord) I may justly pretend, that the things I have set down have been faithfully Recorded, though not elaborately Written; and I suppose my former Papers may have long since satisfi'd You, that though many devise Experiments better than Your Servant, none perhaps has related them more carefully and more truely: And particularly of These; sometimes one, sometimes another hath been perform'd in the presence of Persons, diverse of them eminent for their Writings, and all for their Learning. Wherefore having in the foregoing Narratives made it my businesse to enoble them with the chiefe Requisites of Historicall Composures, Candor, and Truth, I cannot despair that You will either Excuse their Imperfections, or at least Forgive them: Especially considering, that this unpolish'd Letter is as well a Production of Your Lordship's Commands and my Obedience, as a Testimony of my Desire to make others beholden to my Lord of

Dun-

Dungarvan, by the ſame way by which I indeavour to expreſſe my ſelfe

Becon's-fi ld this 20th. of December, 1659.

His Lordſhips

Moſt obedient Servant,

and

Moſt affectionate Unckle,

ROBERT BOYLE.

ERRATA.

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